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The Rhetoric of Science: A Case Study of the Cold Fusion Controversy.

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**The rhetoric of science: A case study of the cold fusion
controversy**

Hatfield, David Lee, Ph.D.

The Louisiana State University and Agricultural and Mechanical Col., 1992

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**300 N. Zeeb Rd.
Ann Arbor, MI 48106**

The Rhetoric of Science:
A Case Study of the
Cold Fusion Controversy

A Dissertation

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Department of English

by

David Hatfield
B.A., Marshall University, 1981
M.A., Marshall University, 1983
May, 1992

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Abstract

This dissertation examines the circumstances surrounding and the rhetoric involved in the cold fusion controversy begun on March 23, 1989, when two University of Utah electrochemists, Martin Fleischmann and Stanley Pons, announced by press conference the discovery of room-temperature nuclear fusion. The dissertation seeks to determine to what extent a rhetorical analysis of cold fusion discourse may increase understanding of the controversy; the success of Fleischmann and Pons as scientific rhetors; the ways in which scientists' attitudes, values, and assumptions manifest themselves in the discourse; and finally, what may be learned about scientific discourse in general by examining the cold fusion controversy in particular. The dissertation employs a method of analysis which combines Lawrence J. Prelli's special theory of scientific rhetoric that identifies relevant issues and lines of argument in scientific discourse, and S. Michael Halloran's method of close textual reading suggested in his study of DNA discourse. Examined were Fleischmann and Pons's initial publication announcing the cold fusion discovery in the Journal of Electroanalytical Chemistry; Steven E. Jones's initial publication of his cold fusion discovery and several representative discourse samples from the journal Nature; and Fleischmann and Pons's latest article in the Journal of

Fusion Technology. Several issues and lines of argument were identified. For the most part, cold fusion discourse addressed evidential issues, questioning the existence of the cold fusion phenomenon. Several lines of argument were evoked to address this issue, including experimental competence, experimental replication, external consistency, communality, and disinterestedness. Also discovered is division between electrochemists and physicists over what constitutes valid evidence: electrochemists looked to excess heat production as proof of fusion; physicists looked to neutron production. The study concludes that Fleischmann and Pons followed an unsuccessful rhetorical strategy in their initial published paper, one that addresses of issue of existence, but their evidence was insufficient to convince as to the scientific reasonableness of the cold fusion claim. An alternative rhetorical strategy was available to Fleischmann and Pons, one in which they could have interpreted, rather than asserted, their evidence, thereby evoking a less confrontational response from the scientific community.

Chapter One: Introduction

Historians, philosophers, and sociologists have, within the last thirty years, begun to renew their attention to and reevaluate the way in which science operates. A prominent reason for this vigorous interest in science seems clear: more and more, science is becoming a fundamental part of everyday life, typically as expanding technology. As S. Michael Halloran observes, scientific matters are increasingly debated in the public arena (once, interestingly, the province of rhetoric):

Science is itself an increasingly public enterprise, both in the sense that the public supports it financially and in the sense that it offers monumental threats and promises to our well-being. Science also serves as warrant for many arguments about traditionally non-specialized, civic questions -- war and peace, ways and means for promoting the public welfare. To understand public discourse in the closing decades of this century, we must have some understanding of scientific discourse. (81)

For similar reasons, Michael S. Overington argues, "it is time that students of rhetoric bend their skills to an analysis of scientific discourse and the creation of scientific knowledge" (143). In this study I shall answer Overington's call to students of rhetoric and attempt to contribute to what Halloran called a need for "a body of critical literature on particular cases of scientific discourse" (81) by applying a working method of rhetorical analysis to a modern scientific controversy.

The particular case I have selected for study came to the public's attention on March 23, 1989. On that day, University of Utah officials held a press conference to announce that two of their researchers, Martin Fleischmann and Stanley Pons, had made a break-through discovery that promised to change the face of the world: sustained cold nuclear fusion. Using simple apparatus, the two chemists had harnessed the power of the sun under room temperature using a fuel source literally as abundant as seawater. The implications of such a discovery were staggering: a cheap, limitless source of energy.

Soon, however, the bright promise of the break-through discovery was dimmed by controversy and questions: Had Fleischmann and Pons acted as responsible scientists? Was what they observed in the lab actually cold nuclear fusion? Why could some scientists report replication while others reported finding nothing at all? What was the relationship between the scientists of the University of Utah and those of rival Brigham Young, who reported a similar discovery? And so on.

To establish an understanding of the cold fusion process and an appreciation for the implications of the Fleischmann/Pons claim, I will first present, in this chapter, a brief discussion of the basic mechanics of nuclear energy, concluding with an explanation of the type of cold fusion that Fleischmann and Pons believe they

created. Also, I will review the circumstances surrounding the Fleischmann/Pons announcement and the events that followed in order to establish a context for the resulting discourse in the cold fusion controversy. Chapter two reviews literature relevant to the rhetorical nature of science. Chapter three presents methodology for the rhetorical analysis of scientific discourse. Chapter four contains the rhetorical analysis of the discourse found in the cold fusion controversy, and chapter five discusses the implications and conclusions of this study.

The Four Types of Nuclear Energy

The discussion that follows is based on my understanding of the nuclear processes after reading various sources, including F. David Peat's eloquent and accessible explanation of cold fusion, and David H. Freedman's article on "hot" fusion.

Nuclear Fission

The form of nuclear energy most of us are already familiar with is nuclear fission. This type of nuclear energy powers electric generating stations and is the type of energy used in the first atom bomb. In the electric generating station, the nuclear reaction both is controlled and sustained. In the atom bomb, uranium atoms are sundered by a brief explosion that results in an immediate release of energy.

In nuclear fission, the nuclei, or centers, of uranium atoms are encouraged to split. In this process, a part of the mass of the nucleus of the atom is converted into a tremendous amount of energy.

Aside from the source of the energy, a nuclear power plant operates much like one fueled by coal or oil. The heat, in this case generated by splitting uranium nuclei, converts water into steam, which in turn propels turbines, which in turn generate electricity. Generating electricity by using nuclear fission is a cleaner process than burning fossil fuels, is not limited by scarcity as are fossil fuels, and is commercially viable. A real concern, however, is what to do with the highly radioactive waste, an unavoidable by-product of nuclear fission.

Nuclear Fusion

Somewhat surprisingly, this type of nuclear energy is less familiar but is not an uncommon occurrence. In fact, right now more nuclear fusion is taking place than nuclear fission. However, the conditions under which nuclear fusion occurs takes it out of the realm of everyday experience; nuclear fusion powers our sun, as well as every other star.

Unlike nuclear fission, in which atomic nuclei are split, nuclear fusion is the process wherein two atomic nuclei--specifically, two hydrogen nuclei--actually fuse,

or come together. This fusion, or melding, results in a form of helium and a free neutron. These two products of nuclear fusion weigh much less than the nuclei which combined, and the loss in mass is released as energy, as described by Einstein's famous formula, $E = mc^2$. Simply put, the energy released is equal to the loss in mass, multiplied by the square of the speed of light. Since the speed of light is 186,000 miles per second, before squaring, obviously much energy is released from even the smallest incidence of fusion and is far greater than that produced by nuclear fission (Freedman).

Encouraging hydrogen nuclei to fuse is difficult. At the core of a star, the combination of crushing gravity and incredible temperatures cause hydrogen nuclei to overcome atomic forces, which normally cause them to repel one another due to their like positive charges, and fuse.

Efforts are underway, however, to duplicate and harness the conditions and reactions that power the sun. Obviously, the engineering and theoretical obstacles are many. The basic challenges are heating the hydrogen atoms to millions of degrees and then holding them closely enough together for a long enough time to increase the probability that the atoms will collide and fuse.

Attempts at creating "hot" nuclear fusion have resulted in the construction of the tokamak, a room-sized, stainless-steel, donut-shaped chamber, into which is pumped

plasma-- hydrogen gas so hot that the electrons are stripped free from their atoms, resulting in a hot soup of swirling, colliding nuclei and electrons. Of course, the walls of the tokamak would melt if they came into contact with the plasma cloud--which has been heated to a record 670 million degrees at the Princeton tokamak (Freedman 32)--so powerful electric fields inside the tokamak act as a sort of "magnetic bottle" (Peat 32) to suspend the plasma within while it is heated even further.

To facilitate fusion, an isotopic form of hydrogen is used, deuterium. Deuterium is actually a naturally occurring element, relatively abundant in sea and drinking water: about 1 in every 6,700 hydrogen atoms exist as deuterium (Peat 25). Whereas a hydrogen molecule consists of one proton in the nucleus orbited by one electron (the simplest elemental configuration), deuterium consists of a nucleus of one proton and one neutron orbited by a single electron. Chemically, deuterium is identical to hydrogen in the way it behaves and interacts with other elements. However, the extra neutron plays an important role in forming the by-products of fusion, helium and the free neutron, which weigh less than the two deuterium atoms. This "lost" mass is converted into energy as described above. Deuterium figures prominently in the following discussions of "cold" nuclear fusion.

Muon Catalyzed Cold Nuclear Fusion

In July, 1987, Johann Rafelski and Steven E. Jones (the same Steven Jones who figures in the cold fusion controversy) published an article in Scientific American entitled "Cold Nuclear Fusion." They described a process known as "muon catalyzed fusion," also termed "cold fusion." Actually, the fusion process is "cold" only when compared to the above described process that requires a plasma of millions of degrees. The fusion process described by Rafelski and Jones actually works best at around 900 degrees Celsius.

A muon is a short-lived, electron-like particle that behaves very much like an electron. In muon catalyzed cold fusion, muons are fired into a gas of deuterium and tritium, which is also an isotopic form of hydrogen with two protons and one neutron in the nucleus orbited by a single electron. Tritium, unlike hydrogen and deuterium, however, is highly radioactive and decays relatively quickly. As the muon travels through the deuterium/tritium gas, it collides with orbiting electrons and will eventually displace an orbiting electron, knocking it aside, much like the collision between billiard balls. However, instead of careening off, the muon is captured by the nucleus and replaces the electron, taking up an orbit around the nucleus.

One of the important properties of the muon, however, is that although it is electron-like, its mass is around 200 times greater than that of the displaced electron. Therefore, it orbits the nucleus around 200 times more closely than did the original electron. As a result, the nuclei of the deuterium or tritium molecule are brought about 200 times closer than normal.

As it turns out in the weird realm described by quantum mechanics, there is another way to fuse nuclei. One way, described above, is to accelerate through superheating the nuclei while they are in close proximity, as in the plasma. The fast-moving nuclei collide and thus fuse. Another, less violent method involves holding nuclei very close for an extended time. During this time, there is a probability that a phenomenon known as "quantum tunneling" will take place.

Due to quantum uncertainty, and as described by Werner Heisenberg's Uncertainty Principle, "there is an intrinsic ambiguity in pinning down the specifications of a nucleus" (Peat 46). Normally, nuclei find themselves on opposite sides of a barrier that works to keep them apart. However, because of this "intrinsic ambiguity" concerning the location of a nucleus, there exists a small probability that a nucleus held in close proximity to another for an extended time may actually find itself on the other side of the repulsion barrier. Thus, through quantum tunneling, a

nucleus may pass through the barrier of repulsion, at which point the atomic force that binds and holds together nuclei takes over, causing the nuclei to fuse.

Though muon catalyzed cold fusion holds promise to one day provide a viable commercial energy source, several problems must be overcome. One problem lies in the short life span of the muon, which lasts only for about two millionths of a second. Another lies in the fact that creating the muons requires more energy than is produced thus far in the reaction chamber. Still another problem lies in the number of fusions that a single (short-lived) muon can catalyze. Rafelski and Jones observe that "pioneering research in physics tends to precede applications beneficial to society by one or two generations" (89). Even though a promise, then, muon catalyzed fusion remains a distant one.

Given the theoretical, technological, and practical problems associated with both nuclear fission and nuclear fusion, it is little wonder that Fleischmann and Pons's announcement of successfully generating and maintaining fusion reactions in a laboratory test tube at room temperatures caused such a stir among the scientific and lay community. Following is a description of another method, the Fleischmann and Pons method, of cold nuclear fusion.

Electrochemically Induced Cold Nuclear Fusion

This method is deceptively modest. Its basis is the same process used in many high school chemistry labs for producing hydrogen gas: simple electrolysis.

In the high school lab, two electrodes are submersed in water a small distance apart. When a current is run through the electrodes, the water is reduced to its basic constituents, hydrogen and oxygen. The oxygen moves to one electrode while the hydrogen moves to the other, both to bubble off. In the high school lab, the hydrogen is collected (and then ignited to demonstrate the combustibility of the gas). This process is exactly that used by Fleischmann and Pons, but with different apparatus.

The electrodes in this case consist of platinum and palladium. Palladium is important to the process because of its atomic structure and its affinity for absorbing hydrogen before it can bubble away. These electrodes are submersed into deuterized water, D_2O ; that is, water composed mostly of deuterium molecules instead of normal hydrogen molecules, H_2O . The oxygen travels to the platinum electrode while the deuterium travels to and is absorbed into the open lattice work structure of the palladium electrode. Simply put, the electrolysis continues, packing more and more deuterium into the atomic structure of the palladium. Soon, the deuterium nuclei are packed so tightly by the galvanic pressure for an extended

time that quantum tunneling takes place, resulting in cold fusion of the deuterium nuclei. As in other fusion processes, the products of this cold fusion are light helium and a free neutron, along with a tremendous amount of heat energy, all of which become important in discussions about whether Fleischmann and Pons had actually achieved what they claimed.

Following is a timeline of events surrounding Fleischmann and Pons's announcement of cold nuclear fusion, adapted, updated, and expanded from a timeline developed by Peat (180-182). Many of the events described here are referred to in chapter four as they relate to the rhetoric surrounding the scientific discourse concerning cold fusion. The articles for the rhetorical analysis in chapter four are identified in the timeline by bold-faced type and full bibliographic information.

Cold Fusion Timeline

August 1926 • Paneth and Peters, two German researchers, cause a stir among physicists by publishing a paper reporting the creation of helium from hydrogen using a palladium catalyst (recall that helium is a by-product of fusion). Though nuclear fusion was an unknown process at the time, the conversion

of hydrogen into helium would have proved a valuable discovery.

April 1927

- Paneth et al. retract the claim, having underrated two sources of error involving helium contamination: glass heated in a hydrogen atmosphere yields up absorbed helium, and palladium heated in the presence of hydrogen, but not oxygen, readily releases helium.

March 24, 1951

- Argentine dictator Juan Peron, along with physicist Ronald Richter, calls a press conference, announcing to the world that Argentina had succeeded in controlling nuclear fusion. Eighteen months later, Peron realized he had been hoaxed; Richter was reportedly arrested after having spent \$70 million (Waters).

1972

- Three University of Utah scientists make headlines when they report the creation of an X-ray laser, a project that had baffled many researchers for years. No known physical process existed to explain the phenomenon, and the laser effect worked only about one time out of ten: some scientists reported confirmation while others observed nothing. The evidence was greeted with

skepticism. Later, researchers discovered the evidence was created by an altogether unrelated effect. Physicists laughingly have termed such situations the "Utah Effect" (Pool 420).

July 1987

- Steven Jones of Brigham Young University co-authors a paper, "Cold Nuclear Fusion," published in Scientific American. Jones describes a process of muon catalyzed cold fusion and situates himself in cold fusion research.

Fall 1988

- Martin Fleischmann and Stanley Pons apply for a Department of Energy grant for funds to continue their cold fusion research (their process is described above), having already invested \$100,000 of their own money into the research (Dagani).

December 1988

- Steven Jones acts as one of the reviewers for the Fleischmann and Pons grant. He approaches Fleischmann and Pons through the Department of Energy about collaboration because his own work overlaps significantly with theirs. Jones's approach was to duplicate the conditions of the earth's interior instead of the sun's.

Concentrations of naturally occurring He^3 (a

by-product of nuclear fusion) around volcanoes and oceanic fissures suggested to Jones that the heat of the earth's core was the result of nuclear fusion. Jones and coworkers' experiments dealt with "low-voltage electrolytic infusion of deuterons into metallic titanium or palladium electrodes" (Jones et al, "Observation" 737) that are emersed in a soup of eight metallic salts, such as those found in the earth's interior.

Additionally, Jones offers Fleischmann and Pons the use of a highly sensitive neutron detection device (recall that neutron release is also a fusion by-product). The device, of his own design, is the only one of its kind. No agreement concerning collaboration is reached.

1989

- | | |
|----------|---|
| February | • Jones decides he is ready to publish his results. |
| March 6 | • Fleischmann and Pons, along with the president of the University of Utah, meet with Jones and the president of BYU. Fleischmann and Pons would like another |

eighteen months to work on their project, but both parties agree to publish their work by submitting papers simultaneously to Nature on March 24.

- March 10
 - Pons speaks with the U.S. editor of the Journal of Electroanalytical Chemistry about a manuscript of the cold fusion paper.
- March 11
 - Pons mails the manuscript.
- March 17
 - University of Utah press officials interview Fleischmann and Pons about their cold fusion experiments.
- March 22
 - University of Utah officials announce a major press conference for the next day.
- March 23
 - Fleischmann and Pons announce their discovery of cold fusion at a press conference in Salt Lake City.
 - After hearing at the press conference that Fleischmann and Pons had prepared a manuscript, Jones and his team of BYU researchers submit their manuscript to Nature.
- March 24
 - Fleischmann and Pons submit their own paper to Nature and express irritation with Jones for breaking their agreement.

- A Brigham Young University press release announces the independent discovery of cold fusion by Jones.
- March 31
- Fleischmann meets with a group of physicists at the European Center for Nuclear Research (CERN) near Geneva, Switzerland, to explain cold fusion. He is well received.
- April
- In the following weeks, a flurry of press releases, faxed reports, e-mail messages, and rumors from labs all over the world, including Stanford and Texas A & M, appear to confirm cold fusion. But soon research groups report negative results in which they claim to have seen nothing.
- April 7
- The Utah State Legislature, in a special session, passes the Fusion/Energy Technology Act. Governor Norm Bangerter requests the state to release \$5 million in research funds.
- April 10
- The Journal of Electroanalytical Chemistry publishes a "Preliminary note," the Fleischmann and Pons article about their cold fusion experiments (Fleischmann, Martin and Stanley Pons. "Electrochemically Induced Nuclear Fusion of Deuterium."

Journal of Electroanalytical Chemistry 261

(1989): 301-308). Though the article is brief and contains little information about apparatus and none about control experiments, journal editor W. Ronald Fawcett says that the article is "a very important piece of science...definitely worth publishing as a preliminary communication" (Dagani 14).

April 12

- Pons delivers a lecture, "Nuclear Fusion in a Test Tube?" to around 7,000 chemists at an American Chemical Society national meeting in Dallas. Clayton Callis, ACS president, remarks that physicists' attempts at hot fusion were "too expensive and too ambitious....Now it appears that chemists have come to the rescue" ("Scientific" 605). Pons is mobbed by reporters and has to change motels.

April 17

- Pons announces a fusion reaction sustained for 800 hours in one of his cells.

April 18

- Italian researchers from Frascati announce cold fusion of a different kind, in which deuterium gas is pumped into titanium metal under high pressure.

- April 20 • Nature explains that Jones' paper, received March 23, would be published the next week but that Fleischmann and Pons's, received March 27, would not. The Fleischmann and Pons article received by Nature is a brief version of that which earlier appeared in the Journal of Electroanalytical Chemistry. Asked to amend their text in response to reviewer's questions, Fleischmann and Pons decline in order to "satisfy more urgent work." Nature states that "the non- appearance of the article must not be taken to imply that the experiments...by Fleischmann and Pons are inherently less believable than those of Jones et al" ("Cold Fusion in Print" 604).
- April 26 • Fleischmann, Pons, and Jones appear before the House Science, Space, and Technology Committee. University of Utah President Chase N. Peterson encourages the organization of a national cold fusion center with an initial investment of \$100 million. He also requests from the federal government \$25 million in seed money.
- April 27 • Jones and coworkers' cold fusion paper appears in Nature (Jones, S. E., et al.

"Observation of Cold Nuclear Fusion in Condensed Matter." Nature 338 (1989): 737 - 740). In that same issue, Nature editor John Maddox lashes out at Fleischmann and Pons, saying their claim "is literally unsupported by the evidence" (Maddox, John. "What to say about cold fusion." Nature 338 (1989): 701).

May 1-2

- The American Physical Society holds special late-night sessions at the spring meeting in Baltimore. Physicist Steven Koonin is applauded after he remarks that the excess heat observed by Fleischmann and Pons is more an indication of "incompetence, perhaps delusion" than cold fusion (Lindley 4). Vocal critic Nathan Lewis, of the California Institute of Technology, and W. Meyerhof, of Stanford, attack Fleischmann and Pons on the grounds of poor calorimetry and data interpretation. The poor calorimetry results from Fleischmann and Pons's failure to stir the liquid in their fusion cells during temperature sampling. Jones' claims, however, are well received, due in part to his more modest claims of neutron output and his ability to gather

sensitive neutron flux measurements with the detector that he designed. Fleischmann and Pons do not attend. The "physicists attending were left with the comfortable feeling that fusion was dead, except for the small effects of the sort claimed by the Brigham Young group" (Lindley 4).

May 8

- The American Electrochemical Society warmly receives the news of cold fusion at its meeting in San Diego.

May 18

- A damaging criticism of the gamma-ray spectra collected by Fleischmann and Pons appears in the "Scientific Correspondence" section of Nature (Petrasso, R.D., et al. "Problems with the γ -ray spectrum in the Fleischmann et al. experiments." Nature 339 (1989) 183-185).

May 23-25

- A cold fusion workshop organized by the Department of Energy and held at Los Alamos National Laboratory is attended by scientists from all over the world and is carried by satellite across North America. Fleischmann and Pons are absent. Both negative and positive results are presented, and the scientists fail to reach a definite conclusion about cold fusion.

- June 15 • Scientists announce at a press conference in Harwell, UK that, following extensive experimentation, they failed to establish evidence of cold fusion. They are abandoning their experimentations.
- July 12 • An interim report from the Department of Energy states, "The experiments reported to date do not present convincing evidence that useful sources of energy will result from the phenomena attributed to cold fusion."
- June 29 Fleischmann and Pons respond in the "Scientific Correspondence" section of Nature to the criticisms of their gamma-ray spectra by Petrasso, et al. that appeared in the May 18th issue of Nature (Fleischmann, Martin and Stanley Pons. "Measurement of γ -rays from cold fusion." Nature 339 (1989) 667). In this same section, Petrasso et al. reply to the Fleischmann and Pons letter (Petrasso, et al. "Petrasso et al. reply." Nature 339 (1989) 667-669).
- August • The state of Utah disburses \$5 million to establish a National Cold Fusion Institute there.
- October 16-18 • A workshop entitled "Anomalous Effects in Deuterized Materials," sponsored by the

National Science Foundation and the Electric Power Research Institute, results in a relatively positive report concerning cold fusion and recommends that further study is justified.

- November 12 • The Department of Energy issues a final, skeptical report on cold fusion: "The panel concludes that the present evidence for the discovery of a new nuclear process termed cold fusion is not persuasive." The word "persuasive" was chosen deliberately, remarked co-chair Norman F. Ramsey. "That's not to say it's untrue" (Goodwin 45).
- December • Both the Bhabha Atomic Research Center in India and scientists in Japan report positive cold fusion results.

1990

- January 1 • Pons begins extensive new experimentation involving sixty-four cold fusion cells.
- February 1 • Fritz G. Will is named head of the National Cold Fusion Institute.
- March 28-31 • The first annual Cold Fusion Conference is conducted in Salt Lake City.
- March 29 • A paper in Nature reports that independent measurements taken on Pons's cells produced

no confirmation of a nuclear reaction. In that same issue appears an editorial, "Farewell (not fond) to cold fusion," comparing the search for cold fusion to that for the Philosopher's Stone.

July

• Fleischmann and Pons's article, "Calorimetric Measurements of the Palladium/Deuterium System: Fact and Fiction," appears in the Journal of Fusion Technology (Pons, Stanley and Martin Fleischmann. "Calorimetric Measurements of the Palladium/Deuterium System: Fact and Fiction." Journal of Fusion Technology 17 (1990): 669-679.) In this article, Pons and Fleischmann answer many specific criticisms.

Statement of Purpose

The cold fusion controversy promises a unique opportunity for a case study of scientific discourse and the way in which scientists rely on rhetoric in the "doing of science." This study of the cold fusion controversy argues against Herbert W. Simon's statement that scientific discourse is rhetorical only in the "softer" sciences such as social psychology. No "evidence of a pattern of sophistic practices within the better established sciences such as physics or chemistry" has been offered, he says

(126). However, the process of cold fusion uniquely bridges the "hard" disciplines of particle, atomic, and nuclear physics and electrochemistry.

Additionally, the cold fusion controversy presents an excellent opportunity to examine the extent to which an understanding of the context surrounding scientific discourse may enrich a study of scientific rhetoric.

I will attempt to answer the following questions by analyzing the discourse surrounding the cold fusion controversy: To what extent might a rhetorical analysis of the cold fusion discourse increase our understanding of the controversy? How successful as rhetors were Fleischmann and Pons in convincing their audience of the scientific reasonableness of their claims of cold fusion? To what extent and in what ways do scientists' attitudes, values, and assumptions manifest themselves in the discourse of the controversy? And finally, what may be learned about scientific discourse in general by examining the cold fusion controversy in particular?

Chapter Two: Review of the Literature

Traditionally, scientific discourse has not been considered a text for rhetorical evaluation. The classical tradition maintains a strict dichotomy between science and rhetoric. Aristotle argued that science dealt with demonstrably true certainties, whereas rhetoric dealt with matters of opinion, or the uncertain. James Kelso suggests that rhetoricians have disregarded a rhetorical aspect of scientific discourse based on conclusions drawn from Aristotle: science is not open to deliberation, and the certainties of the natural world are distinct from the contingencies of human convention. Kelso maintains that rhetoricians have traditionally failed to realize that scientific theories may be considered symbolic representations of reality (28).

In the following sections, I will review representative scholarship from the philosophy, rhetoric, and sociology of science, and examine an emerging discipline known as the rhetoric of inquiry. The discussion of these disciplines will demonstrate how the rhetoric of science is bound to and draws upon each: in their communicative practices, scientists draw on knowledge of how science operates, how scientists interact, and how scientists conduct inquiry. Finally, I briefly address the epistemological implications of a rhetoric of science. In

each of these sections, I show that persuasive discourse is an integral component of scientific practice.

The Philosophy of Science

The past thirty years have seen an active exchange of views among the modern philosophers of science, ranging from the highly rational, empirically based views of science of Sir Karl Popper to what has been called the irrational, "mob rule" view of scientific activity held by Thomas Kuhn. Despite much debate among the philosophers of science, there is no consensus concerning which theoretical approach in the philosophy of science is the best to pursue to accomplish two goals: to provide a framework for the further development of effective scientific theories and to provide a model that scientists may follow to ensure that their theoretical pursuits will result in the best contribution to scientific knowledge. While the views of the major philosophers of science differ greatly, all the views may be reconciled with the notion that scientific communication is rhetorical. The philosophies I review here include those of Sir Karl Popper and his theory of falsificationism, Imre Lakatos and his method of the scientific research program, and Thomas Kuhn and his theory involving paradigms, normal science, and revolution in the development of scientific theories.

Sir Karl Popper is a most influential philosopher of science, and perhaps the champion of the rational approach. Conjectures and Refutations, originally published in 1962, gives the foundation for his method of falsification, and Objective Knowledge, revised in 1979, presents his epistemic theories concerning scientific practice. Rejecting from the beginning the notion of theory verification, Popper asserts that the falsification of theories is the only rational procedure for developing scientific theory. Popper agrees with philosopher David Hume, who casts much doubt on induction, asserting that no argument may establish

'that those instances, of which we have had no experience, resemble those, of which we have had experience.' Consequently, 'even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience...why from this experience we form any conclusion beyond those past instances, of which we have had experience.'
(Conjectures 42)

Therefore, verifying a theory is impossible because one cannot logically conclude that because past tests have confirmed a theory, future tests will do so likewise.

At this point, both Hume and Popper are left with an important problem: if induction is illogical, how then do we account for our amassing knowledge? How is our knowledge obtained? Popper suggests that we actively try to impose regularities upon the world. We try to discover similarities in it and to interpret it in terms of laws

invented by us. Without waiting for premises, we jump to conclusions. These may have to be discarded later, should observation show that they are wrong.

This was a theory of trial and error--of conjectures and refutations....conjectures boldly put forward for trial, to be eliminated if they clashed with observations; with observations which were rarely accidental but as a rule undertaken with the definite intention of testing a theory by obtaining, if possible, a decisive refutation. (Conjectures 46)

The scientist, then, should proceed as follows: state a theory that may be tested through observation and name before any testing the conditions or outcomes by which the theory may be considered as falsified, or refuted. Naming the conditions beforehand that will falsify the theory demarcates, for Popper, the scientific theory from the pseudo-scientific (some theories, such as some astrological theories, can never really be said to be true or false, for it is difficult to name precise conditions under which such theories may be said to be wrong or false) and establishes a rational means of scientific progress based on integrity and disallowing any ad hoc adjustments to make theories coincide with observed data.¹ Thus, Popper's method places much importance on the critical experiment, that

¹Popper recognizes that a theory may always be adjusted to make it fit experimental outcomes. Ad hoc adjustments are those that allow the theory to escape refutation without adding any empirical or predictive content, that is, counters experimental refutation but does nothing to increase the theory's predictive power or expand its set of logical consequents. This idea of excess content is also significant in Lakatos's philosophy.

particular, crucial test that may either help confirm or falsify a theory.

Popper's method of science, therefore, offers a rational means to proceed with the development of scientific theories while recognizing problems with verificationism. Popper's views on falsification raise some interesting questions on the function and nature of rhetoric should one implement his proposed method of science. The exchange of critical discourse between scientists is an essential element of Popper's method of conjectures and refutations. States Popper,

Faced with a certain problem, the scientist offers, tentatively, some sort of solution--a theory. This theory science accepts only provisionally, if at all; and it is most characteristic of the scientific method that scientists spare no pains to criticize and test the theory in question. Criticizing and testing are complimentary activities; scientists criticize the theory from many sides to reveal weaknesses. (Conjectures 313)

The rational aspect of this process enters in when these vulnerable points are subjected to the severe scrutiny of scientific testing. Ultimately, for Popper, the process is not one ruled by rhetoric or argument, but rather by scientists basing their theories and accepted knowledge on good reasons, good reasons derived by the refutation or confirmation of theories based soundly in experimental

outcome and observation.² Yet, one sees the obvious problem of denying the role of rhetoric considering Popper's reliance on critical discourse. Obvious questions arise when one asks "Which of these good reasons is the best reason?" and perhaps more significantly when one asks, "When are the conditions of falsification met: how much refutation is enough and how does one prove or communicate this refutation?" As shown in the timeline presented in chapter one, for example, many scientists reported confirmation of Fleischmann and Pons's findings while many others found no evidence of cold fusion. While it may be possible to pursue a method of falsification as Popper suggests, just when the actual theory has been falsified returns to the realm of critical discourse; the ultimate decision by the scientific community then must rest not only on rational experimental outcome but, ultimately, upon how one presents these outcomes through critical discourse and communication.

Marilyn Schauer Samuels suggests further implications of Popper's method on the rhetoric of scientific communication in her article "Technical Writing and the

²This idea of "good reasons" will play a central role in the method of analysis discussed in chapter 3. The good reasons are in part the set of values and norms established by a community of scientists by which they critically judge discourse. I will suggest that how well or how poorly particular discourse addresses these good reasons (the rhetorical element of discourse) influences success or failure of theories.

Recreation of Reality." Her thesis is that technical writing is a more creative venture than is commonly thought, and that when writing, technical writers create their particular versions of reality.

Though Samuels slightly misrepresents Popper, her conclusion is nonetheless valid. Popper does not, as she suggests, advocate the "falsifiability of reality," but, more precisely, the falsification of theories about reality, for Popper advocates an objective reality revealed through experimentation when our theories are falsified. Also, Popper does not "advocate the on-going revisions of reality as the aim of science," but rather that truth--in the ultimate, objective sense--is the aim of science, and that we move closer to that truth by uncovering the points in our theories where an objective reality asserts itself by disproving conjectures about its nature. Samuels is correct, however, in concluding that our conjectures and refutations will result in an evolving depiction of reality. Scientists, as do the technical writers in her thesis, depict their own versions of reality when positing theories and debating these versions through critical discourse.

Closely related to these ideas are the themes of Popper's Objective Knowledge, which demonstrate the scientific statement's significance in the practice of science. By objective knowledge, Popper does not mean

knowledge produced without bias or subjectivity. Rather, for Popper, the scientific statement exists independently of a knower, just as a collection of books in a library exists independently of a reader. Further, this knowledge would continue to exist even after humanity or anyone else who could read it had long since vanished; so too does the body of scientific statements exist. These statements have an existence independent of reality, and it is upon this body of knowledge that we apply our experimentations and scrutinies.

It is not reality, then, that is subject to falsifiability, but, as Charles Bazerman points out, it is the scientific statement or theory that becomes the object of scrutiny upon which critical operations may be performed and the method of falsifiability applied (Scientific Writing 159). The implication is, as suggested, that Popper's method of falsifiability is not applied to reality; it is removed one step from the critical operations and direct observations of the laboratory and experimentation and applied to the scientific statement itself. Thus, the significance of the scientific statement is revealed in Popper's method.

Another widely read and influential philosopher of science is Imre Lakatos. His "Methodology of Scientific Research Programmes" outlines another rational approach to the development of scientific theories and knowledge.

Lakatos disagrees with Popper on some fundamental points, but builds his methodology on the idea of falsificationism--of a particular sort--and the particular application of this method, not to a single theory, but to a series of theories, or research program.

Lakatos maintains that it is unreasonable that a scientist would abandon a theory because some observational facts contradict it. There must be some greater reason for the scientist to abandon one theory for another. Lakatos suggests that scientists move on to other theories only when a better theory--"better" as defined by predicting novel facts and possessing excess content--is offered. For Lakatos, there is no falsification before the emergence of a better theory (35). Until that time, scientists will continue testing the current theory. The problem is shifted, then, from one of appraising theories to appraising a series of theories, or research program. Lakatos maintains that great scientific achievements are not outcomes based on hypotheses but on scientific research programs.

A research program is built around two types of heuristics. The negative heuristic, or "hard core," forms the set of major, underlying assumptions that are no longer tested by scientists because such tests no longer yield novel results. For instance, a research program may be established around Newton's three laws of motion. The

three laws themselves are unquestioned, so experiments that test the three laws would not be considered fruitful experimentation. The positive heuristic constitutes the set of auxiliary hypotheses surrounding the hard core and functions as a sort of "protective belt." For instance, if a theory predicts outcome O and $\sim O$ occurs, the positive heuristic is adjusted; the assumptions of the negative heuristic are not questioned. It is possible that the positive heuristic may in this manner undergo a problem shift, scientists in the program focusing on different research questions while maintaining the negative heuristic as the guide to valid research (48-49).

Programs that yield theories which predict novel facts or even generate new problems for investigation he terms progressive research programs. Programs that generate theories which lack the ability to predict novel facts and only accommodate previously unknown facts are termed degenerative research programs. Thus, progressive programs are characterized by predictive power, degenerative programs by only explanatory power. Research programs survive despite the rise of anomalies so long as they can generate new problems. When the program struggles simply to explain anomalies, scientists begin to follow more promising programs. Thus, scientific progress is realized not through falsification (because anomalies accompany any

theory) but by progressive programs replacing degenerative ones.

Still, the importance of discourse and the rhetorical implications remain inherent in Lakatos's philosophy. As Bazerman suggests,

In order to communicate the point and value of new work, the scientific writer would be well advised to understand how his or her new contribution fits within the continuity of the problems of the relevant research program. If Lakatos is right, adherence to accepted theory is not so necessary for an article's gaining acceptance as is adherence to the current research program....Thus an article that expands a field's problems or redirects the research program is more consequential for the development of a science than the critical experiment that would presumably falsify one theory and verify a competing theory. (Scientific Writing 160)

To ensure success, then, scientific writers should learn strategies for demonstrating that their work is in line with the current program while showing explicitly how their work opens new lines of investigation and thought. Papers that are interpretative in purpose, suggesting new explanations for known anomalies, should be especially well received. However, interpretative papers that question the negative heuristic would be less well received.

Scientists' reluctance to radically shift lines of thought or investigation is revealed in the rhetorical analysis of the cold fusion discourse, as well as evidence that scientists resist interpretations that require an adjustment to the negative heuristic. Part of Fleischmann and Pons's lack of success in their handling of cold fusion

may be attributed to their failing to successfully convince their audience that their evidence demonstrated a need to re-think current fusion knowledge and lines of investigation.

Thomas Kuhn's The Structure of Scientific Revolutions is perhaps the most popular account of scientific philosophy, particularly among humanities scholars. This popularity is due in part to Kuhn's likening the development of science to that of other, more humanistic fields. He describes scientific development "as a succession of tradition-bound periods punctuated by non-cumulative breaks" (Structure 208). Thus, Kuhn presents a revolutionary view of science based on his idea of the disciplinary matrix.

Kuhn agrees with Popper in that much of science proceeds by putting forth hypotheses that are systematically tested in attempts to falsify them. However, Kuhn terms such a view "a virtual cliché" ("Logic" 4). Though such hypothesis testing constitutes "the activity in which most scientists inevitably spend almost all their time" (Structure 6), Kuhn extends the notion by placing such testing within a particular social structure in the community of scientists and describing it as a particular type of scientific activity.

Particular communities of scientists are united by assumptions that the scientists have some idea of how the

world works (Structure 5), what Kuhn terms the disciplinary matrix, "disciplinary, because it is common to the practitioners of a specified discipline; matrix, because it consists of ordered elements which require individual specification" ³ ("Logic" 271). The disciplinary matrix includes "shared symbolic generalizations;...shared models;...[and] shared values" ("Logic" 271-72). The disciplinary matrix defines a particular scientific community or specialty and also defines valid research problems and methods for solving those problems.

Thus, scientists involved in a particular community spend most of their time practicing "normal science," that is,

research firmly based upon one or more past scientific achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. (Structure 10)

Kuhn has likened this period of normal science, that is, the typical procedure of scientists, to puzzle-solving. Unlike Lakatos, Kuhn believes that "the most striking feature of... normal research problems...is how little they

³Kuhn coined the term "disciplinary matrix" as a replacement for his term "paradigm," used in The Structure of Scientific Revolutions. Masterman criticized Kuhn's use of the term paradigm, counting "not less than twenty-one different senses" in which the term was used (Masterman 61). Kuhn's switch to disciplinary matrix was intended to "unravel confusions...that are entirely of my own making" (Kuhn, Criticism 271). The many senses of the term paradigm are subsumed under disciplinary matrix, defined in the text above.

aim to produce major novelties, conceptual or phenomenal" (Structure 35). Much of the puzzle-solving work is intended to "add to the scope and precision" of the already accepted views of the disciplinary matrix (Structure 36).

Because of this puzzle-solving nature of normal science, anomalies usually present little threat to the disciplinary matrix. Typically, it is the individual scientist's own ingenuity, not the current theory or disciplinary matrix that is tested. If a particular conjecture fails the test, it is the scientist's "own ability not the corpus of current science [that] is impugned...for in the final analysis it is the individual scientist rather than current theory which is tested" ("Logic" 5). Additionally, normal science progresses rapidly because scientists only focus on those "problems that only their own lack of ingenuity should keep them from solving" (Structure 37).

The puzzle-solving activities of normal science have implications for scientific discourse as well. Philosophy and many social sciences have been characterized by critical discourse among the practitioners, discourse concerned with claims, counterclaims, and debates over fundamentals ("Logic" 6). However, during normal science within a disciplinary matrix, such debate over fundamental values and exemplars would be immaterial. According to Kuhn,

it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy. Only when they must choose between competing theories do scientists behave like philosophers. ("Logic" 6)

In Kuhn's philosophy, not all science, however, is normal science; such moments of crisis as mentioned above do indeed arise. These periods he refers to as revolutions, when one disciplinary matrix is given up in pursuit of another.

Kuhn explains why this shift occurs:

Sometimes a normal problem, one that ought to be solved by known rules and procedures, resists the reiterated onslaught of the ablest members of the group within whose competence it falls. On other occasions a piece of equipment designed and constructed for the purpose of normal research fails to perform in the anticipated manner, revealing an anomaly that cannot, despite repeated effort, be aligned with professional expectation. In these and other ways, normal science repeatedly goes astray. And when it does--when, that is, the profession can no longer evade anomalies that subvert the existing tradition of scientific practice-- then begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science. The extraordinary episodes...are...scientific revolutions. They are the tradition-shattering complements to the tradition-bound activity of normal science. (Structure 5-6)

Periods of scientific revolution are the consequence of normal science's puzzle-solving activities. Though the aim of normal science is not to produce novelties, its activities are very effective in generating them. As Kuhn explains,

normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way....Without the special apparatus that is constructed mainly for anticipated functions, the results that lead ultimately to novelty could not occur. And even when the apparatus exists, novelty ordinarily emerges only for the man who, knowing with precision what he should expect, is able to recognize that something has gone wrong. (Structure 65)

Such revolutions are infrequent, momentous, and associated with names such as Copernicus, Newton, and Einstein, each of whom's theories "necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it" (Structure 6). This rejection of one disciplinary matrix "is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other" (Structure 77).

Additionally, one characteristic of the new disciplinary matrix is that its view of the world is incompatible with the former matrix. For instance, one could not view the solar system as both geo-centric, as did Ptolemy, and helio-centric, as did Copernicus; Newton's three laws do not describe Einstein's world of relativity. Kuhn explains further that in the shift from one matrix to another,

words change their meanings or conditions of applicability in subtle ways. Though most of the same signs are used before and after a revolution--e.g. force, mass, element, compound, cell--the ways in which some of them attach to nature has somehow changed. Successive theories

are thus, we say, incommensurable. ("Logic" 266-67)

The implications for rhetorical discourse in Kuhn's theory of science are many. Though Kuhn states explicitly that "it is precisely the abandonment of critical discourse that marks the transition to a science" ("Logic" 6), I believe that he refers to debate concerning the shared values, assumptions, and exemplars of the disciplinary matrix, not the specific outcomes of testing done under normal science. Indeed, it is the individual scientist's own ingenuity and apparatus that is in doubt when conjectures fail. As I demonstrate in chapter three, scientists frequently debate whether the correct apparatus was used, whether the data were correctly interpreted, whether the proper theory was evoked to interpret the data, whether the most valid and reliable method was used, and so on. These issues might arise when scientists question whether the shared values, methods, assumptions, and exemplars defined by the disciplinary matrix were applied correctly. That is, while the content of the matrix is unquestioned, the application of its content may be.

Further, Kuhn suggests that during periods of revolution, critical discourse plays a central role, scientists behaving much like philosophers as they debate the merits of competing matrices. Bazerman observes that it is during periods of revolution that the nature of the discourse changes most:

At the height of revolution, writing should take on a markedly argumentative, persuasive character. There should be clear evidence of miscommunication between members of the two matrices. The character of writing within a disciplinary matrix also should change as it loses or gains hold, either entering or leaving a period of revolution. Finally...a writer at a time of revolution would be wise to direct comments not so much at his opponents as at uncommitted third parties, such as young scientists entering the field; the argument should proselytize rather than attempt a definitive answer to the opposition. (Scientific 161-62)

All three of the above philosophers, Popper, Lakatos, and Kuhn, describe science in ways that differ in assumptions and epistemology. Popper's rational approach of falsificationism; Lakatos' programs of scientific research; and Kuhn's theory of normal science governed by the disciplinary matrix that evoke periods of revolution all demonstrate the importance of scientists' identifying relevant issues and addressing them persuasively--that is, satisfactorily--in communications with other scientists.

Rhetoric of Science

Much rhetorical scholarship of late has dealt directly with the idea that scientific discourse has a rhetorical element. In this section I will review some representative ideas to establish the current thinking about how scientific discourse exhibits rhetorical characteristics.

James A. Berlin identifies several twentieth century rhetorical positions, two of which embody the dichotomous

stances described by Kelso above. Berlin describes the first such rhetorical stance as an "objective rhetoric...based on a positivistic epistemology." This stance appears in twentieth century thinking in what Berlin terms "current-traditional rhetoric" (Rhetoric 7). In this rhetorical theory, reality exists in a material world, and truth is arrived at "through collecting sense data and arriving at generalizations." The observer's role is one of objectivity, "necessitating the abandonment of social, psychological, and historical preconceptions that might interfere with the response of the faculties to the external world." Truth exists prior to and separately from language, which can be either a "distorting mirror" when reflecting external reality or, preferably, "a transparent device that captures the original experience," objectively reproducing truth "conceived exclusively in empirical and rational terms, with emotion and persuasion relegated to oral discourse" (Rhetoric 8). From this particular stance, scientists are viewed as producers of "truth" and "scientific fact."

Berlin describes a second rhetorical stance, the New Rhetoric, or Epistemic Rhetoric ("Contemporary" 773). In this rhetoric, language is present in every element of the communicative transaction: "interlocutor, audience, and material world are all regarded as verbal constructs." In this stance, "there is never a division between experience

and language....All experiences, even the scientific and logical, are grounded in language, and language determines their content and structure" (Rhetoric 16). Thus, "all truths arise out of dialectic, out of the interaction of individuals within discourse communities" (Rhetoric 17). From this position, scientists are viewed as creators of probable knowledge: "knowledge is not simply a static entity available for retrieval. Truth is dynamic and dialectical, the result of a process" of communication ("Contemporary" 774). Language, then, plays a central role in scientific knowledge because "the elements of the communication process....form the elements that go into the very shaping of knowledge" ("Contemporary" 774).

Both the current-traditional and epistemic rhetorics make obvious a relationship between epistemological stance and a rhetoric of science. The former would seem to preclude a rhetorical element in the creation of scientific knowledge; the latter opens many possibilities. However, as will be made more obvious throughout this literature review, both the current-traditional and epistemic rhetorics can be shown to agree with a model of scientific practice that includes a rhetorical element. This agreement is discussed explicitly in this chapter's treatment of epistemological concerns when it is shown that a rhetoric of science can play a central role in creating scientific knowledge even while maintaining a realist

epistemology. One can conclude, then, that a rhetoric of science is not an epistemology of science, because a rhetoric of science can agree with many epistemological stances. The implications of this conclusion are discussed at length in chapter five.

Epistemology aside, Herbert W. Simons's verdict is that "The donkey [science] is rhetorical through and through, but still capable of carrying a heavy load" ("Rhetors" 126). He points out that in our society, the term "rhetoric" has bad connotations, described by words like "mere, only, empty, or worse," so scientists understandably avoid an association. In the classical, non-pejorative sense, however, rhetoric connotes "reason-giving activity on judgmental matters about which there can be no formal proof" ("Rhetors" 127). This classical sense permits and even encourages the concept of rhetoric as good reason-giving on matters of judgment.

Simons decides that this latter sense is what promoters of science mean by "scientific objectivity" ("Rhetors" 128). Popper, Simons points out, suggests that what sets scientific discourse apart from ordinary rhetors is this "error-correcting process of exchange between scientists" ("Rhetors" 116). Popper calls for the unrestrained criticism of any and all ideas, and so insists that theories be stated in a manner in which they can be falsified or tested ("Rhetors" 116). Simons concludes,

then, that the rhetorical practices of scientists are kept in check by Popper's notion of free criticism.

Simons further observes that the rhetorician also can aid science in this error-correcting process in serving as critic of discourse. Simons suggests that

rather than carping at science in general, rhetorical critics would be more persuasive were they to reserve their slings and arrows for more limited and vulnerable targets such as individual practitioners or communities in the time-bound sense. Of special concern should be willful violations of scientific canons, and here critics can stand not as enemies of science, but as defenders of its time-honored norms. ("Rhetors" 128-129)

From a perspective "friendly" to science, he concludes that "there is also much theoretical work to be done" ("Rhetors" 129). In the cold fusion controversy, Fleischmann and Pons violated several standards of scientific behavior, such as the announcement by press conference rather than peer-reviewed medium. How these violations may have contributed to the controversy is an important rhetorical concern.

Scholars of rhetoric have found much evidence that writers of science employ rhetoric in their communications to establish the importance of a line of research, to demonstrate how their findings fit with accepted standards of investigation, and to undermine opposing theoretical interpretations. For example, Carol Reeves, in her study of reports in the New England Journal of Medicine that announced the discovery of immunodeficiency in homosexual patients, found that writers use rhetorical strategies to

emphasize the importance of their claims and suggested lines of investigation. The writers she examined first established AIDS as a mysterious new phenomenon, presenting symptoms that current medical knowledge was unable to explain. Later, though, the writers relied on existing knowledge to explain the phenomenon, demonstrating that the immuno-deficiency caused by the virus was not surprising under the circumstances and may in fact be solvable. This strategy brings to mind Kuhn's theory, above, in which he suggests the appeal that puzzle-solving has for scientists, but also the requirement that the puzzle appear solvable through the scientists' ingenuity. Reeves further points out that establishing and validating a new phenomenon in a given community or discipline is an important question for rhetorical study. She cites sociologist Robert Merton who identifies the "political dimension" involved in the competition to be the first to characterize and establish a phenomenon: "With varying degrees of intent, groups in conflict want to make their interpretations the prevailing one of how things were and are and will be" (395). This political dimension arises in the cold fusion controversy as well, for the attempt to be first to lay claim to the cold fusion phenomenon is one possible reason why Fleischmann and Pons made their initial announcement by press conference.

This theme of competition and its manifestation in rhetoric arises again in Andrew J. Weigert's article, "The Immoral Rhetoric," and also indicates the relationships between the philosophy, sociology, and rhetoric of science. He cites Kuhn, who suggests that "the triumph of theory depends on the number of practitioners who are persuaded to utilize it." Thus, he argues, a theoretical rhetoric arises in scientific papers that is aimed at weakening the explanatory power of rival theories. For instance, in research articles, the literature is selectively reviewed to emphasize corroborative findings. The need to persuade and answer objections from others in the discipline imperceptibly transforms the exponent of a theory into a proponent of it. To invest one's intellectual life in the exposition of a theory is to communicate an evaluation of it. Theoreticians who claimed that their theories were no better than others would open themselves to questions concerning their reasonableness and credibility within the community of scientists. The observing community assumes that the scientist believes in the theory; otherwise, why would so much time be invested in it? Thus, even the exponent of a theory seeks to persuade the readers (113). Weigert concludes that "whatever its scientific rationale, reconstructed logic functions rhetorically as a form of persuasion....If the reconstructed logic is successful, the

scientific act is persuasively presented in a way in which it did not in fact take place" (113).

In his article "Rhetoric and Science: Notes for a Distinction," Don Geiger echoes Kelso by suggesting how, after the classical stance, that rhetoric and science are distinct enterprises, yet can be made complementary to one another. In a sense, rhetoric is "a means of communication of some value in the solution of certain kinds of social problems" (54). Rhetoric is the means of identifying truth with feelings; or, as Aristotle suggested, "Rhetoric...is essentially the art of giving effectiveness to the truth" (54). Science, then, is to find the truth; rhetoric, to energize it. "Rhetoric," Geiger contends, "has widened its scope, and instead of being regarded as a narrow art of persuasion, is now concerned with the proper presentation of all knowledge" (54).

David Edge and Nigel C. Gilbert found that scientists also use rhetoric to show that their work fits with and agrees with current lines of research and knowledge. They examined an objectified, quantitative analysis of scientific communication called the citation analysis, which studies the references used by writers. Though citation analysis seems at first to belong more to a discussion of the sociology of science, this review of both Edge and Gilberts' work will reveal that a quantitative analysis of scientific interaction fails in part because of

the rhetorical nature of scientific discourse, specifically a persuasive use of referencing, or citation.

Edge asserts that proponents maintain that "citation analysis has been demonstrated to be a valid and valuable means of creating accurate historical descriptions of scientific fields" (109). Two uses of citation analysis are, one, the determination of influence of one or more researchers on another researcher. A will cite B and C and so on. Such citation indicates that B and C influenced A's work. A second use is the determination and identification of what Diana Crane has termed "invisible colleges," social groups of scientists with similar interests, research methods, and aims.

The proponents of citation analysis contend its benefit is systematic and objective analysis, one that may even eliminate the human element from such analysis. For instance, all data found in Science Citations could be entered into a computer, which would then be programmed to do all the groupings and analysis. Edge criticizes this notion of purely objective analysis. For example, by taking the analysis out of human hands, a participant's own accounts that B's work was not influential on his or her own would either have to be ignored or the citation analysis would have to be corrected.

Additionally, Edge suggests that there exists both formal and informal communication among scientists; that

is, communication may take place that references do not record. The formal communication upon which citation analysis depends "is the tip of the iceberg" (113). Further, Edge raised the issue that a major problem with citation analysis is the persuasive use of citation, which Nigel C. Gilbert explores.

Gilbert also examined citation analysis. He identifies three uses to which citation analysis has been put: using the number of citations a paper or researcher receives as an indicator of significance or "worth"; mapping disciplines or specialties; and constructing typologies of varieties of reference and citation by content analysis of the citations. Gilbert points out that while some interesting contributions have been made by such analyses, care should be taken when dealing with such because no explicit or accepted theory underlies their use, and thus no reasoning exists that can explain why some authors cite others. Gilbert offers some ideas, however.

Current explanations about why scientists use citation as they do explains inadequately why scientists often cite 1) references that are "negational in character (that is, those references to papers which the author wishes to challenge or contradict" and 2) references that seem not strictly relevant to the author's present research. However, if one were to consider the scientific article as persuasive and assume that the scientist who has obtained

results he believes to be important and true must convince his colleagues of such--and by that share his opinion of the value of his work--such questions as the above are more satisfactorily answered. Gilbert suggests that references help demonstrate the validity and significance of the work presented in scientific papers.

To gain recognition for new findings, scientists must demonstrate that 1) their work represents an advance on past research; 2) their findings relate to current research in the field; 3) their work is free of error and uses proper theories and techniques; and 4) alternative, contradictory hypotheses have been considered and discarded. Some of the above are accomplished through argument and inference detailed in the body of the paper; the latter suggests a clear relationship between scientific and persuasive discourse. Citation of references, since they have already been incorporated into the body of valid knowledge, represents a measure of persuasive support (116). It follows that it would be more persuasive to cite an accepted paper than to redescribe the research without reference to the original paper.

Scientists, then, use those respected papers to bolster acceptance, give justification, and demonstrate the novelty of their work, as well as indicate how one's own work illuminates or solves problems raised by previously accepted work (116). Thus, scientists tend only to cite

papers they decide that their audience will consider "important and correct" in established fields, even if the citation is not directly related to one's own work. Gilbert's arguments recall Lakatos' philosophy, which suggests that to be successful, scientists must demonstrate how their work fits into and advances the research program.

Charles Bazerman analyzed research articles dealing with spectroscopy that appeared from the late 1800's to present. His most interesting findings were about the changing epistemology of the articles and how this change was evident in the rhetoric of the discourse. Bazerman concludes that

the large-scale trends revealed...are consistent with the traditional view that science is a rational, cumulative, corporate enterprise, but point out that this enterprise is realized only through linguistic, rhetorical, and social choices, all with epistemological consequences. (Shaping 183)

While early articles focused on methodology, more modern writing takes on a more interpretative and problem-solving posture, thus opening the possibilities for scientific rhetoric.

For example, an 1893 article that Bazerman examined employs a rhetoric based on empiricist epistemology. The main concern of the article is with methodological techniques, which are discussed in great detail (Shaping 177).

In 1950, a new style of argument appears that will be more fully developed in a 1960 article: the modelling

approach. Epistemologically, the modelling approach recognizes a split between nature and theory, theory being a human construction, having no reasonable expectation of giving a full or accurate account of nature (Shaping 181). Under this approach, a paper can only propose a model that accounts for the data better than other models. In terms of argument structure, a modelling article does not present a claim at first to be explained, supported, and discussed in light of experimental data; instead, once the article locates the problem in relevant theory and presents appropriate data, only then does it offer its model or claim about what apparently occurred in the experiment. Results are first presented, then puzzled over. Only after the puzzlement is the provisionally best model offered (Shaping 181).

Once the argument moves away from notions of absolute truth and error, the concept of fit between theory and data becomes more important. Thus, 1970 finds articles locating their problems in the deteriorating quality of fit between theory and data; a new theory gains ground because it improves that fit. The experiment is designed to find the cause of the discrepancy. The article ends by calling for new theory and experimental work (Shaping 181).

By 1980, articles have become increasingly tentative about the certainty and epistemological status of their claims (Shaping 181). For instance, the spectroscopic

community became gradually more organized and unified by the common bond of theory and theory testing; it relied on the common theory to produce experimental problems and relied more upon and referred more to each others' work. "As quantum mechanics became more established, it provided a coherent organizing principle for work and argument, and the practicing spectroscopist had to attend to the relationship between his own work and others "and show how his work fit in to the current research program" (Shaping 181). This concept is central to Imre Lakatos's philosophy of science, discussed above, in which programs of research flourish when its member scientists identify new avenues of thought and investigation.

As theory grew, Bazerman observes, it became apparent that it was a construction, separate from the nature it described, and this awareness affected argument as well as social relations (Shaping 183).

This review of rhetoric of science scholarship reveals the importance of rhetoric in the practice of science. For example, the rhetoric and philosophy of science are strongly linked. Scientists look to the way science operates to structure their writing and inform their rhetoric, using rhetoric to accomplish important tasks as defined by the philosophy of science, such as redirecting current lines of thought and research. Also apparent is a relationship between epistemology and rhetoric; as Bazerman

suggests, epistemological assumptions influence and are reflected in the rhetoric. Further, the modern sense of rhetoric employs a wide applicability, becoming enmeshed with the way scientists produce and properly present truth. The next section will examine the relationship of the rhetoric of science with the sociology of science.

Sociology of Science

The social forces at work within specific scientific communities, or specialties, can influence the form, content, and dissemination of scientific discourse. Discussion of the philosophy of science has suggested the importance of scientists' identifying relevant issues and addressing them in their discourse. These issues are products not only of the philosophical aspects of doing science but the social as well. Kuhn's philosophy especially emphasized the importance of the shared values and assumptions of the disciplinary matrix. This matrix may be defined not only philosophically, but also sociologically as the values and standards upheld by the particular community of scientists. Thus, the idea that successful communicators in science must identify and address the issues and concerns of the specific community is not exclusive to philosophy. For instance, Medwar discusses the possibility that the scientific research article may misrepresent the thoughts

that gave rise to it (42). The form that the paper takes is meant to reflect a particular procedure of scientific activity, so that, in effect, it demonstrates to the scientific community that the writer actually followed the procedure valued by that community (even though the writer may not have). He posits that the prototypical research article embodies a mistaken, yet pervasive, concept of the nature of scientific thought.

For example, the structure of the research article is introduction, previous work, methods, results, and then discussion. In the results section, for instance, one must refrain from discussing the importance of the findings and pretend that the mind is an empty vessel ready to receive information (a practice which Fleischmann and Pons failed to follow, as discussed in chapter four's analysis). In the discussion section, the writer must "adopt the ludicrous pretense of asking yourself if the information you have collected means anything" (42). The underlying concept is a scientific process in which generalizations grow from simple, unbiased, unprejudiced observation: from an unordered collection of data, an orderly, general statement will develop.

However, all scientific work begins with an expectation--a hypothesis--about the outcome of an inquiry. This hypothesis governs the shape of the inquiry, even though in their genesis, "hypotheses arrive by guesswork"

(43). In light of this expectation, some outcomes are deemed relevant and others are not; some methods are chosen and discarded; and some experiments done rather than others. This behavior is apparent in the cold fusion experiments by both Fleischmann and Pons and Jones. Fleischmann and Pons, as electrochemists, arranged 2 of their 4 experiments to measure excess heat while failing to look for other kinds of evidence of fusion. Jones, a physicist, looked only for neutrons, but one type of fusion evidence, using a neutron detector of his own design. Apparent in these experimental designs are underlying assumptions about relevant experiments and evidence.

Considering this general behavior, Medwar concludes that the inductive format of the scientific paper should be discarded; the discussion should come first, and then the data, and scientists should not be ashamed to admit, as they apparently are, "that their hypotheses appeared uncharted in their minds," and that they are "imaginative and inspirational in character, that they are indeed adventures of the mind" (43).

Not only is form influenced by social forces within a scientific community, but also content, especially when one substitutes the concept "audience" for scientific community, as Michael A. Overington has done. His thesis is that scientific activity "may be treated rhetorically because the construction of scientific knowledge involves

argumentation before an audience" (144). Overington posits four stages in the production of scientific knowledge that link four rhetorical concepts to the sociology of science:

1. The education of young scientists transforms them into licensed speakers about matters that concern their communities.
2. Scientists engage themselves in research situations that are necessary conditions for their speech.
3. As a result of their engagement in research, scientists construct and publish arguments which offer plausible reasons to their audiences for judging the conclusions of these persuasions to be valid.
4. Over periods of time scientific audiences provide authoritative judgment on the status of these arguments as scientific knowledge. (154)

Chapter three discusses the importance of and manner in which scientific writers, as identified in three, above, "offer plausible reasons to their audiences for judging the conclusions of these persuasions to be valid," an argument essential for a rhetoric of science.

Bazerman suggests that, regarding writing, the constraints and opportunities provided by the specific genre and style at any particular time and within a particular discipline are determined by a collection of social factors. The individual writer, in making decisions concerning persuasion, must write within a form that considers the audience's current expectations of appropriate writing within that field. These expectations provide resources as well as constraints, for they provide guides to the formulation of an argument, and suggest ways

of presenting material that might not have occurred to the writer's imagination otherwise (Shaping 165).

Bazerman further contends that the conventions of writing within a discipline are as much a product of that discipline as are its knowledge claims (Shaping 166). Moreover, since the institutional arrangements of writing conventions directly affect the symbolic representations that constitute knowledge, writing conventions help create the very thing called "knowledge" (Shaping 166). How a discipline decides to communicate with itself, what it presents as potential contributions to knowledge, and how it conceives and argues for those potential contributions are essential parts of how a discipline constitutes itself in fulfillment of its task of creating knowledge (Shaping 166).

Lyne and Howe also suggest that distinct fields of discourse exist within the sciences and can complicate communication. Because researchers within different specialties hold different assumptions, a scientist's venture beyond the strictest confines of a research specialty will sometimes lead to misunderstanding (133). Key misunderstandings may arise when writers attempt to graft rhetorical constructs of one specialty onto another (139). Whether miscommunication between disciplines occurred in the cold fusion controversy is an important consideration for investigation. Pons and Fleischmann are

electrochemists; many critics of both the experiments and experimenters were nuclear and particle physicists. If Lyne and Howe are correct, one would expect to find some degree of miscommunication between members of the different specialties. This question is explored in-depth in chapters four and five.

Frank E. Millar also addresses the theme of knowledge creation and argues that science is part of a critical approach to knowledge. The scientific knowledge claim is an expression of organized experience; that is, it is repetitive, repeatable, redundant, predictable. Thus the quest for creating knowledge--which is always created, not discovered--is the quest for organizing experience. The elucidation and communication of these patterns is what makes knowledge and education possible. Millar contends that "the societal function of science is no less than the description of social illusions people live by at any point in time....Scientists do not deal with truth; they deal with limited and approximate descriptions of reality" (227).

W. R. Albury found that not only are scientific theories sometimes scrutinized in the academic forum, but often in the public forum as well. For example, the Sociobiology Study Group (SSG) of Science for the People attacked E. O. Wilson's theories that dealt with a genetic basis for racial differences, comparing them to Naziism.

Though the scientific value or validity of Wilson's genetic theories did not enter into any criticisms, the fact that his study dealt with racial differences based upon genetics was enough to bring the theory into disrepute.

This issue of criticism directed at theories or scientists by those outside the field is addressed in chapters four and five. Fleischmann and Pons show concern about the many negative comments directed at them by the press ("Calorimetric" 669); negative editorials about cold fusion and Fleischmann and Pons also appeared in Nature, often alongside original research articles. Since research scientists were just as likely to see articles skeptical of cold fusion as they were confirming or non-confirming research articles, one wonders how such criticism by the journal's non-scientific community influenced those scientists.

Philip C. Wander asserts that "the archetypal speaking situation for the scientist occurs in addressing an audience of fellow scientists, and the archetypal form of discourse is the research report" (230). He argues that the scientific report must do more than just give information; it must persuade as well. For instance, the scientist must first convince the editorship of the journal that the subject is worthwhile, that the scientist knows what he or she is talking about, and that the findings can

be integrated into current knowledge (230), echoing Lakatos.

Such an argument further raises questions about the publication process's influence on scientific discourse and its distribution. Diana Crane, in her article "The Gatekeepers of Science," states that the norms of scientific conduct require that a scientist's social characteristics be left out of the publication decision. However, she points out studies that suggest that the evaluation of scientific articles is not entirely objective, that a scientific stratification system acts differentially to distribute publication opportunities to particular scientists, opportunities that play an important role in a scientist's success.

For instance, Crane asserts that some studies suggest that many published articles are written by scientists at major universities, suggesting that selection of articles is influenced by academic affiliations. One 1945 study showed that articles from authors at minor universities were rejected more frequently by The American Sociological Review than those by authors at major universities.

Crane found further that the distribution of characteristics such as academic affiliation, doctoral origin, and professional age of contributors was essentially the same distribution as those among the journal editors. She suspects that most instances,

however, are due not to explicit prejudices but to editors responding to characteristics in the writing of those with similar academic training, methodologies, theoretical orientations, and modes of expression (200).

This conclusion confirms the idea that has pervaded the discussion on philosophy and sociology thus far: that communities of scientists share certain values, concerns, and assumptions. Discourse, then, which reflects and successfully and persuasively addresses these shared values, concerns, and assumptions should be more likely to meet with success than discourse that ignores and/or fails to address such issues. One can imagine, then, a rhetorical method of scientific discourse that could identify heuristics that scientists could follow to increase the likelihood of acceptance of a research article, both by colleagues and journal editors. One could imagine further that, armed with such a heuristic, a rhetorical critic may analyze how well or how poorly a scientist has identified relevant issues and addressed them in his or her discourse. These imaginings I attempt to make substantial in chapter three of this study.

Alfred de Grazia further emphasizes the role of the scientific reception system, and thus the importance of scientific discourse, in the doing of science. He goes so far as to suggest that "when a scientist writes a book of his controlled experiences, a publisher ponders its

audience, and a colleague weighs its value, the special order of human relations called science is in being" (45). The behaviors of the individuals involved follow common human patterns, and so do their motives: one can't separate the scientist from the person and the person from society and its behaviors at large. Likewise in science, as in every social order, there exists a reception system. The system in science is the same as in any social order: it "shapes the character of the new recruits to the order and therefore forms the product of the order" (45). He suggests, as does Crane, that journal editors are the "gatekeepers" of science, screening the information that is permitted to circulate widely among members of the discipline and tending to support the currently orthodox views in their fields. Their receptivity to new ideas is variable.

De Grazia characterizes several reception systems, one of which he terms the Rationalistic Reception System, based on the scientific method. "Its goal is truth, enlightenment, knowledge, or just simply 'science'" (46). The model itself denies a sociology of science; the idea that scientists are conditioned by social factors that lie outside their intellect is dismissed in this view: an objective method exists for testing reality, and anyone confronted with the truth will recognize it immediately. In this rationalistic doctrine, the rule of the publication

holds primary importance: every scientist has the right to publish and should review results with peers before submission.

Several other rules are at work in the Rationalistic Reception System. One holds that works must be read before judgment is passed. Another holds that theories offered should be tested, not only by their authors but by their critics. Honesty and fairness are also a credo. "Unless scientists are willing to admit the source of their knowledge and theories, and willing to grant a fair hearing and test to ideas brought forth, they contribute to the collapse of the rationalistic reception system" (48). De Grazia further observes that "the model of rationality demands that the populous be barred from scientific proceedings...the implication being that unless a work has the previous blessings of the scientific establishment, it has no right to exist" (49).

If de Grazia's conclusion is correct, Fleischmann and Pons seem ill-fated from the start, having violated several rules of the Rationalistic Reception System. Their results on cold fusion underwent no sort of peer review before being made public; their theories were thus untested by critics; Pons and Fleischmann refused to release their apparatus for investigation, nor did they release enough detail to allow other scientists to replicate their experiment; and news of their findings was made at a

public press conference rather than in a scientific journal. Needless to say, their work had not earned, as de Grazia states, "the previous blessings of the scientific establishment." De Grazia would conclude then that their work had "no right to exist." The details of Pons and Fleischmann's behavior and the possible implications of their violating nearly every rule of the rationalistic reception system will be discussed at length in chapters four and five.

Rhetoric of Inquiry

"The Rhetoric of Inquiry will change the way we think about the way we think," concludes Herbert W. Simons after attending the four-day 1984 University of Iowa symposium on Rhetoric and the Human Sciences. An emerging discipline, the rhetoric of inquiry concerns itself with the use of rhetoric in the communication practices of research disciplines of all kinds, whether scientific, humanistic, or mathematical. This discipline is based on the assumption that all fields of human inquiry have as their basis argument and persuasion. As Nelson states,

Scholarship uses argument, and argument uses rhetoric. The 'rhetoric' is not mere ornament or manipulation or trickery. It is rhetoric in the ancient sense of persuasive discourse. In matters from mathematical proof to literary criticism, scholars write rhetorically. Only occasionally do they reflect on that fact.
(Rhetoric 3)

Further, a central concept is that rarely, if ever, do facts speak for themselves: "facts themselves are mute, [scientists] do the speaking-- whether through them or for them" (Nelson, Rhetoric 8). If scientific discourse is at the root persuasive, argumentative communication, a clearer understanding of the functions of rhetoric in scientific fields will "make the sciences more intelligible to themselves and others" (Nelson, Rhetoric 5). In their article "The Rhetoric of Inquiry: Projects and Prospects," Nelson and Megill engage in what they term "an exercise in communication across fields, an attempt to explain and advance a line of investigation they call rhetoric of inquiry" ("Projects" 20). They relate briefly the story of the thinkers prior to the seventeenth century who regarded rhetoric as the prime domain of language and argument. Since then, rhetoric has played a small role in Western thought. They observe that rhetoric has become "at best a technology useful to politicians or others who wish to win friends or influence people, but trivial in its own right" ("Projects" 20).

The rhetoric of inquiry urges scholars to overcome any insistence on certainties either needed or achieved. Nelson and Megill suggest that in scholarship, as with all human activities, accepting uncertainties can often produce a greater appreciation of questions and complexities. In inquiry, such a stance allows an understanding of the

diverse standards and strategies of the sciences on their own levels. The rhetoric of inquiry insists on connecting the process of science not only to its logic and method, but additionally to its aesthetics, economics, histories, and sociologies. Insisting that even academic reasoning is practical reasoning, it situates research in uncertain but actual communities of human experience. "It reminds scholars that rhetoric was the first, and in some respects, remains the foremost science of the human communities" ("Projects" 25).

The rhetoric of inquiry has the power to dispel dichotomies among the humanities and social sciences by comparing different arenas and strategies of inquiry, thus helping independent projects learn from the results and principles of others (35). It may encourage scholars to radically rethink their disciplines by revealing the ways in which the sciences rely not less upon but differently upon rhetoric than do the humanities. It should not be a distinct field, although specialization is inevitable. It must grow and gain strength from and be informed by the interaction of disciplinary research and inquiry, interaction that is necessary to ensure the quality of its analysis and the authority of its advice ("Projects" 35). Its goal is to explain how scholars legitimately invoke different reasons persuasively in different contexts (35).

John Lyne describes the 1984 Iowa Symposium on Rhetoric and the Human Sciences in his article "Rhetorics of Inquiry." Lyne reports that while no single definition of rhetoric was evident at the symposium, four identifiable lines of thought emerged from the symposium by which one may characterize the function of rhetoric in relation to academic inquiry.

1) Rhetoric as Argument: Rhetoric may be considered the argumentative practice particular to each academic paradigm, the stuff of which argument is made and which is then taken as a source of knowledge within the several human sciences (66). One area of research is whether the sources of disagreement in the human sciences differ in kind from those issues found in the hard sciences. Lyne asserts that the persistence of disagreement among equally competent experts confounds the model of inquiry as simple mastery of the craft (67).

2) Rhetoric as Configuration: The literary studies' approach to rhetoric is reemerging, sharing with the communication field "the common ground of narrativity, metaphor, and figuration" and owing to contemporary theories of meaning that have undermined the literal/figurative and fact/fiction dichotomies (69). If one grants a world socially constructed, "constituted of arbitrary signs in ideological formations read like a text...there is little to distinguish between literature

and the discourses of 'the real world'" (69). The manner in which academia builds its knowledge claims through figurative language is a growing area of study.

3) Rhetoric as Critical Practice: The oft-voiced concern for what happens to objectivity if one concedes a persuasive role to rhetoric in the structure of inquiry is just one part of the broader concern about error and accountability. No one should deny the possibility of being somehow subject to correction. Any speech act should be accountable to the world in which it is produced. Criticism is one method to maintain that accountability, and it is a function of rhetoric. Correction and accountability need not presuppose fixed standards; an ongoing, evolving dialogue among research paradigms will demand both (69).

4) Rhetoric as a Means of Empowerment: Scholar Gerald Bruns has observed that rhetoric originated as a means of enabling people to gain power over situations (70). Perhaps the greatest benefit of studying the rhetoric of inquiry will be its ability to explain somewhat the relationship between power and knowledge. We might gain a better understanding of how power relations are enhanced, changed, or diluted by the rhetorical development of "knowledge" (70).

Lyne suggests the study of any of the above areas of rhetoric might contribute to an overarching purpose of

making academic discourses better serve public needs, "enriching rather than denuding the grounds of public understanding and decision" (70).

How might one recover wisdom in a modern, technological age in which the various research specialties shield themselves from public judgements through use of self-isolating vocabularies and territorial claims of expertise? If rhetoricians can begin to get a sense of the rhetorical practices of these specialties, it may be possible to "make the discourses of academic knowledge better mesh with public life" (70-71).

Simons also offers his commentary on the symposium, writing that it "may well be remembered as a watershed event in the history of rhetoric" ("Chronicle" 52). Panelists included McCloskey, an economist; Megill, a historian; Nelson, a political scientist; Thomas S. Kuhn; a science historian and philosopher; and Richard Rorty, a philosopher. Those attending experienced a shared excitement and agreed they were attending an event whose time had come.

The symposium challenged several objectivist notions: the correspondence theory of truth; the mind as a "glassy essence"; scientific language as a mirror of reality; and verification as well as falsification as demarcation criteria for a "true" science. Some panelists, such as Richard Rorty, questioned the notion of epistemology itself

("Chronicle" 53). The challenges cut across many disciplines: history, women's studies, literary criticism, theology, and law. These challenges to long-held and prevailing notions have resulted in intensified critical attention to the language and logic of the human sciences that lead inevitably to their use of rhetoric ("Chronicle" 53). Geertz referred to a "refiguration of social thought" ("Chronicle" 53), both here and abroad, catalyzed by a shift from technology imageries to metaphors and analogies taken from the humanities. In the "new metalanguage" of the human sciences,

behaviors, cultures, entire historical epochs might be viewed as texts, scientific data as symbolic constructions, scientific theories and descriptions as narratives, mathematical proofs as rhetorical tropes, and the ongoing activities of scientific communities as conversations. ("Chronicle" 53)

Richard Rorty is the philosophic spokesperson for the rhetoric of inquiry. In his piece "Science as Solidarity," he accurately describes our culture as one that considers synonymous the concepts of science, rationality, objectivity, and truth. As such, science becomes a religion of sorts: the scientist replaces the priest as the one who promises to deliver abstract, ultimate truth. As a result, any discipline that wants to be taken seriously must imitate science. Disciplines with names like "human science" or "behavioral science" or "political science" arise and seek ways to incorporate the rationality and objectivity of scientific methods into their own.

If science is to be emulated, however, it should not be because scientists are "more 'objective' or 'logical' or 'methodical' or 'devoted to the truth' than other people'" (45). Rather, it is because science is a "model of human solidarity" (46). The "goal of inquiry...[is] the attainment of an appropriate mixture of unforced agreement with tolerant disagreement" (48). It is the atmosphere that promotes free and open debate that fuels human inquiry. A more productive pursuit to advance human knowledge is possible through a new mind-set achieved through a shift in vocabulary that reveals that scientists should be emulated because of the way they utilize free and open criticism.

For instance, a popular notion of scientific rationality naming beforehand the criteria by which a hypothesis will be declared disconfirmed (recall Popper's philosophy of science) and be prepared to discard the hypothesis under such conditions. However, problems arise from this thinking. For instance,

It is characteristic of democratic and pluralistic societies to continually redefine their goals. But if to be rational means to satisfy criteria, then this process of redefinition is bound to be nonrational. So if [this process] is to be viewed as rational...rationality will have to be thought of as something other than the satisfaction of criteria which are statable in advance. (40)

However, if we then define rational in its other sense, as "sane" or "reasonable," rather than "methodical," the term

represents a set of moral principles: "tolerance, respect for the opinions of those around one, willingness to listen, reliance on persuasion rather than force" (40). When thought of as such, a dichotomy between the rational and irrational has little relevance toward distinctions between hard and soft sciences. It has more to do with a method that "eschews dogmatism, defensiveness, and righteous indignation" (40). It has to do with blurring the distinctions between objective and subjective and fact and value that the criterial definition of rationality promotes. "Truth," suggests Rorty, is what will win out in an atmosphere of free and open debate, an atmosphere that promotes the moral principles referred to above. For "objective," we should substitute "unforced agreement," for included in such a substitute is the guarantee given for any "objective" observation, namely, intersubjective verification. Thus, Rorty concludes that "the best way to find out what to believe is to listen to as many suggestions and arguments as you can" (46).

Epistemological Questions

Suggesting that the practice of science is, at least in part, rhetorical eventually raises epistemological concerns: What, then, is the status of existing knowledge claims? How can we then ever hope to discover truth or what is real? How do essentially subjective,

individualistic arguments interface with the objective universe?

Such questions retain as their basis the traditional dichotomy between rhetoric and science, especially evident in questions such as the latter. What is becoming clearer is that through rhetoric scientists are able to reach a consensus about what is reasonably true about the workings of nature.

At the outset, questions about epistemology and ontology may be addressed most easily by arguing that such concerns are actually a red herring: the manner in which scientists convince as opposed to of what they convince are different issues. My examination in chapter four of the rhetoric involved in the cold fusion controversy is not intended to answer the question, "Is cold fusion a reality?" or "Is what Pons and Fleishman observed actually cold nuclear fusion?" Rather, the goal of my study is to explain the use of rhetoric in scientific communication and the "doing of science." Whether scientists make direct observations of reality, whether they have access to directly observable phenomena through undistorted views, and whether the truths we hold as truths are true are issues apart from what strategies scientists use to present evidence and convince others of its scientific reasonableness.

However, wish as we might, such deep rooted and historically popular issues are not so easily laid to rest. Some argue that accepting the premise that science has a rhetorical element presupposes a certain epistemological bent. For instance, the logical positivists such as those of the Vienna Circle, foundationalists who grounded knowledge claims in observable, empirical hard facts, would accept no such notion. Other more "soft" or "fuzzy" thinkers like Richard Rorty, who would substitute the term "unforced agreement" for objectivity, readily endorse the concept of a rhetorical face of science. James A. Berlin earlier identified two twentieth century rhetorics with dichotomous positions on the role of language in reality.

To further complicate the matter, some programs of rhetorical inquiry make epistemological claims. Nelson and Megill argue that the program espoused by the rhetoric of inquiry is "a conceptually adequate and pragmatically useful replacement for foundationalism and objectivism" (Hikins and Zagacki 201). Simons remarks that the rhetoric of inquiry "will change the way we think about the way we think." And Bazerman's analysis of the epistemological trends in spectroscopic articles demonstrates how epistemological stance influences an article's rhetoric. Hikins and Zagacki offer the finest theory I have encountered to reconcile the scientists' search for truth while allowing for their use of rhetoric. Hikins and

Zagacki offer their theory as an attenuation of the rhetoric of inquiry's program. They object to the rhetoric of inquiry's subjectivist emphasis, claiming its program is "grounded on erroneous and inconsistent assumptions" and objecting that such interpretations "prevent an adequate account of past progress in the sciences, philosophy, and the arts" (201). Hikins and Zagacki argue that the rhetoric of inquiry "abandons the distinction between epistemology and ontology, resulting in untenable consequences" (212).

They further criticize the rhetoric of inquiry on several levels. First, they argue generally against the notion of the "value-laden hypothesis," prevalent in sociological views of science and philosophies similar to Kuhn's. Such views argue that scientists have values, goals, and preconceptions that necessarily affect their hypotheses and observations of experimental data and outcomes. Kuhn, for instance, argues that there is no theory-independent way of testing reality. This view, argue Hikins and Zagacki, is a non sequitur. They concede that scientists, like all other people, have values, goals, and so on, but this concession "in no way entails that observation is systematically or necessarily distorted, or otherwise rendered opaque to understanding. Indeed, this conclusion is merely assumed to follow by most who rely on the argument" (209).

Second, Hikins and Zagacki attack a notion central to the rhetoric of inquiry's program and exemplified by Rorty's concept of "unforced agreement": intersubjective truth, "that audiences decide upon their own standards of discovery and decision-making, and what counts as 'true' for such audiences is whatever they agree upon by consensus....With the dissolution of objectivist ontology comes the kindred dissolution of any external standards by which to evaluate...the beliefs of rhetorical communities as 'false'" (212-213). Therefore, they argue, the Flat Earth Society's beliefs about a truly flat earth or the community of children's beliefs in Santa Claus are as valid as any other belief. Hikins and Zagacki rebut the argument for intersubjective truth thusly: no matter how many or how strongly children believe in Santa Claus, he will not really exist (213).

Thirdly, Hikins and Zagacki realize that the result of the program's replacing symbolism for realism and representationalism leaves it open to the same criticism it levels against realism and representationalism, because the symbols it substitutes actually represent real-world concepts such as "rhetors, audiences, messages, objects of discourse, and goals of edification" (217).

They conclude that

So thoroughgoing is the skepticism resulting once this realization obtains, that [this view] devolves into a solipsistic conundrum....If we hold the skeptic to his/her assumptions, no

audience can ever possibly comprehend the intent of the expositor's discourse, and no expositor can ever hope to gauge successfully the audience's reasons for interpreting his/her message as it does. Communication appears hopeless. Obviously, skeptics must be making some tacit, if not a priori, assumptions about the rhetorical nature of rhetorical situations....There is, in short, no consistent way for such a position to give an account of language and communication which does not simply undermine its own rejection of objectivity. (218)

Hikins and Zagacki "suggest that the only way to preserve as meaningful and useful such seminal communication concepts as audience, speaker, message, and rhetorical environment is to renounce representationalism in all its forms," including the above discussed symbolism, the idea that communication operates through symbols that distort or even create reality (218). To do so, one must reject the assumption "that the objects of reality--things-in-themselves--are necessarily opaque to human understanding" (218). What they offer as an attenuation to the rhetoric of inquiry's program they term "rhetorical perspectivism."

According to this view, conflicting or various depictions of reality are not so much the product of subjective distortions as they are the products of the viewer's perspective on a particular aspect of reality. For instance, one observer of a high hill may report that the hillside is covered with deciduous trees while another may report the hillside is covered with pine trees. Though

at first these reports may appear contradictory or even mutually exclusive, they are both accurate when one considers perspective: the two observers are looking at different sides of the same high hill (219). No logical problems are involved; no epistemic problem exists; and the disparate reports are reconciled through a process of communication: once both campers explain their positions--that is, allow one another to "stand inside the other's perspective"--both are enlightened (220). Thus, though the view is a realist view, the role of rhetoric remains central, for in mediating perspectives, "language does not stand as a distorting intermediary between rhetor/knower and the object of discourse/knowledge. Instead, language serves as an interface, making possible a linkage between consciousness and the furnishings of the external world" (222). Hikins and Zagacki can thus salvage realism and objectivism and maintain the centrality of rhetoric in the doing of science. Their rhetorical perspectivism meets the "scientific standards" of elegance and parsimony and seems a valuable and reasonable mediation of realism and rhetoric, demonstrating that a rhetoric of science can play a central role even in realist world-views.

This review of the literature has suggested most importantly the inter-relationships among the rhetoric of science and the philosophy and sociology of science, the

rhetoric of inquiry, and epistemology. Scientific rhetors draw upon and are influenced by all these disciplines, for evident is the importance of identifying relevant scientific issues and concerns and addressing these satisfactorily for the audience. Additionally, many specific issues and concerns from these disciplines were identified as factors in the cold fusion controversy, especially from the philosophy and sociology of science. For example, the philosophy of science suggests the importance of Fleischmann and Pons's demonstrating the relevance of their claims to current lines of thinking and inquiry; Fleischmann and Pons violated many sociological norms of scientific behavior. Chapter three will develop a working method whereby the rhetorical critic can identify these relevant issues and concerns that confront scientific rhetors in their attempts to deal successfully with these elements in their discourse.

Chapter Three: Methods of Rhetorical Analysis

Chapter two asserts the central importance of communication between scientists in their practice of science. An examination of scholarship from the philosophy, rhetoric, and sociology of science reveals the importance of scientists first identifying relevant issues from their particular communities and research programs and then addressing these issues satisfactorily. Successful scientific writers may demonstrate how their work fits into and advances the current program of research; they may solicit support for their particular disciplinary matrix; they may attempt to describe a new phenomenon and lay claim to a particular research avenue; or they may argue strongly that one theoretical orientation is more fruitful than some other.

The obvious task, then, in order to judge the degree of the scientific writer's success, must also be to identify the relevant concerns of the scientific community and various research programs. S. Michael Halloran has more clearly identified this task: "the job of the rhetorical critic is to discover what in the particular case were the available means of persuasion, and judge whether the rhetor managed them well or badly" (70).

In this chapter, I will develop a working theory of rhetorical analysis by examining a method developed by

Lawrence Prelli in his book, A Rhetoric of Science: Inventing Scientific Discourse, and by reviewing Halloran's rhetorical analysis of DNA discourse. Using Prelli's method to identify relevant issues and lines of argument and Halloran's method of what I term "close reading" to judge how well these issues and arguments are addressed, I will apply in chapter four this working theory of rhetorical analysis to the cold fusion controversy. Because Prelli's is the more formalized of the two methods, I first discuss his in some detail and then turn to Halloran's method through an examination of his analysis of DNA discourse.

Prelli's Special Theory of Rhetorical Invention

Lawrence Prelli asserts that when scientists communicate with one another, "they make a special kind of rhetoric" (1). There is more to science than the logical dimension; he discusses McMullin who argues that the logicity of science is but one aspect of science. The logical positivists of the Vienna Circle erred when they insisted that science proceeds only on the basis of formally logical criteria. Such an insistence elicited criticism from many sources. As discussed in chapter two, for instance, Popper successfully undermined the notions of induction and verifiability. Kuhn argued that formally logical procedures occurred only during what he termed

periods of "normal science." Prelli also makes the point emphasized in chapter two that all the aforementioned theories of the philosophy, sociology, and history of science and the rhetorical programs of inquiry, when "taken collectively, make clear that science has an other-than-formally-logical 'face'," which McMullin has termed the "interpretive face" (Prelli 3).

In sum, this interpretive face is intuitive, irreducible to explicit criteria, context dependent, reliant on individual talents, and often controversial (Prelli 3). It follows that if science has an interpretative face, the activity of doing science is necessarily rhetorical, involving "arguments that are informal, material, contextual, and controversial" (Prelli 5).

Prelli goes on to argue that to study scientific communication, one must establish a valid and reliable theory of scientific rhetoric. A rhetoric of science is not actually the science, but must indicate the logic that the science exhibits. It also is not a philosophy, history, sociology, or psychology of science, but again must agree with the tenets of such. A rhetoric of science should explain comprehensively the communicative practices of a science: the discursive decisions scientists make when attempting to "render their claims reasonable in the eyes of scientific audiences" (Prelli 8), and the way

scientists decide between competing theories. Such scientific decisions are based on "good reason," that is, the "choices will be made with due respect for the values and knowledge problems shared by that [scientific] community" (Prelli 113). Claims can be considered reasonable depending on how well they are judged to "possess qualities of empirical accuracy, consistency, quantitative precision, or explanatory power." Claims which lack these qualities "can be dubbed scientifically unreasonable" (Prelli 115).

Scientists, however, while sharing a variety of values, can still debate reasonably about the relative importance and applicability of these values in a given situation. In such cases, the "logicality of persuasive success depends on whether those claims can be judged situationally reasonable" (Prelli 114). This view of scientific rhetoric agrees with traditional definitions of rhetoric and their concern with persuasion in the given situation. Further, such a view

avoids the strains and struggles of accounting for scientific activity as fundamentally either rational or irrational, objective or subjective. It is also consistent with what actually happens in scientific exchanges. Audiences of scientists judge scientific claims, not with reference to the canons of formal logic, but against received community problems, values, expectations, and interests. The judgmental standards are located within situated audiences' frames of reference, not in logical rules that transcend specific situations for scientific claiming. (Prelli 7)

The goal, then, of a rhetoric of science should be to reveal "the principles according to which scientific discourse is created and judged" in order to "reveal significant aspects of scientific endeavors that might otherwise be concealed" (8).

Prelli has developed such a rhetoric of science. His definition is adapted from Aristotle, "the faculty of observing in any given case the available means of persuasion" (35), and Burke's idea of symbolic language, that rhetoric is "the suasive use of language as a symbolic means of inducing cooperative acts and attitudes in symbolizing [human] beings " (13-14).

Prelli has identified the methods by which scientists use rhetorical invention as a means of constructing successful scientific rhetoric. As established earlier, these methods are also useful for the analysis of scientific rhetoric. Prelli has developed a matrix of stasis procedures and distinguished several scientific rhetorical topoi appropriate to scientific discourse. Both of these aspects of rhetorical invention promise to be useful tools for a case study of cold fusion.

Rhetorical Stases

Prelli's matrix of the rhetorical stases--or issues--of scientific discourse identify particular points of contention in discourse. Prelli observes that "points at

issue in science always concern one or more of problems about existence, meaning, value, and action" (147). These four points of contention he terms superior stases, and they

identify arguable points about the four grand functions of doing science: adducing evidence, interpreting constructs and information, evaluating the scientific significance of matters discussed, and applying scientific methods. Discussants must agree on such matters or scientific activity cannot expand comprehension of natural order. (Prelli 145)

Included under each of these superior stases are subordinate issues that must be reconciled; Prelli labels them subordinate stases. The sixteen stases and their concerns are shown below in figure 1.

For a scientific rhetor's discourse to seem scientifically reasonable, he or she must address one or more of the issues involved in the superior stases; the audience may already be concerned with the issues, or the rhetor may raise them. Sometimes a single piece of discourse may address all four superior stases. For instance, scientists may question whether enough evidence exists to present a definite judgment on some new phenomenon. Some may argue that the evidence is an instrumental artifact or is inconclusive (evidential). Others may contend that the models used for explaining the phenomenon are more important than the certainty of the evidence (interpretative). Yet others may argue that the evidence is insufficiently scientifically valuable to

pursue (evaluative); and someone else may contend that no more evidence is needed but a new method of collecting evidence is (methodological) (Prelli 147).

SUPERIOR STASES

SUBORDINATE STASES	EVIDENTIAL	INTERPRETATIVE	EVALUATIVE	METHODOLOGICAL
Conjectural	Is there scientific evidence for claim x?	Is there a scientifically meaningful construct for interpreting evidence?	Is claim x scientifically significant?	Is procedure x a viable scientific procedure in this case?
Definitional	What does the evidence mean?	What does construct y mean?	What does value z mean?	What does it mean to apply procedure x correctly?
Qualitative	Which empirical judgements are warranted by available evidence?	Which interpretive applications of construct y are more meaningful?	Which claims are more significant, given value z?	Which investigations exemplify appropriate applications of procedure x?
Translative	Which evidence more reliably grounds claims about what does and does not exist?	Which scientific constructs are more meaningful?	Which scientific values are more significant?	Which procedures more usefully guide scientific actions?

Figure 1. Rhetorical Stasis Procedures of Scientific Discourse (Prelli 146)

Also, a paper may address all four subordinate stases within a superior framework. For instance, in a paper announcing a discovery, the scientist might address the evidential stasis to establish that the new phenomenon does indeed exist. Discourse addressed at establishing the

reliability of the evidence or that the evidence is not an instrumental artifact would involve the evidential/conjectural stasis; addressing questions about the meaning of the evidence or fitting it into existing taxonomies involves the evidential/definitional stasis. Demonstrating that the evidence proves a conclusion instead of merely suggesting probability addresses the evidential/conjectural stasis, while determining which claims about reality seem the most likely based on the evidence addresses the evidential/definitional.

On the other hand, in dealing with a new phenomenon, writers might place their discourse within the interpretative framework, determining which scientific models are most useful for illuminating the evidence. Therefore, put into the situation of writing about new phenomenon, the writer is faced with a choice: does enough evidence exist to render a definite judgment that will seem scientifically reasonable? Or should one simply suggest a construct for interpreting the new evidence? Whatever the situation, however, the important characteristic of all the above situations, for both the scientific writer and the rhetorical critic, is the abundance of choice.

As mentioned in the introduction to this chapter, Prelli offers his stasis procedures primarily as a method for creating scientific discourse but also recognizes its value as a means of analysis. By making scientists and

rhetorical critics more formally aware of this already informally recognized procedure, perhaps then, as Nelson and Megill suggest, the "sciences would become more comprehensible to themselves and others" (Rhetoric 7).

Rhetorical Topoi

In addition to his matrix of stasis procedures, Prelli has also identified several rhetorical topoi, or lines of argument, which one would reasonably anticipate a scientific audience to raise in a given situation. Prelli contends

(1) that there are in fact identifiable lines of thought that are used again and again in the sciences; (2) that these lines of thought legitimize scientific observations and claims because they derive from what is accepted and valued in scientific communities; and (3) that if we want to see what the logical formulas and characteristics of scientific discourse are, we must grant that these topoi identify structures of thought that scientists (and often others) find situationally reasonable. These themes constitute a stable, ever-present collection of discussable options....Knowing what these options are is helpful to any scientist deciding what to say and how to say it to his or her colleagues. Knowing them is also useful in critique of scientific rhetoric because the array of standard topoi will remind observers of what could have been said about a scientific subject. With knowledge of these options a creator or critic of scientific rhetoric is able to estimate the relative usefulness or nonusefulness of themes in specific rhetorical situations. Knowledge of legitimated themes is also knowledge of what topoi will not count as reasonable, scientific thought; for example, the topos of "inspiration" is not in the array of topoi authorized by scientific communities. (Prelli 216-217)

A method of rhetorical topoi, then, would yield a heuristic which one could follow to generate ideas about topics that may be raised by a particular audience and that would help persuade that audience. Such a method would generate categories, questions, or relationships between ideas perhaps not readily evident to the rhetor otherwise.

Topoi differ from stases in that stases are issues of dispute and questions of relevance, while topoi are lines of argument which one may use to address the issues. For example, the stasis, or issue, may be whether or not an experimental result was actually observed; the rhetor may include the topos of significant anomaly to help convince others of the importance of the finding. In practice, a rhetor (or rhetorical analyst) could identify the issues in discourse, and then consider which of the several lines of argument could be used to address the issue.

Considering the goal of science--to explain, predict, and control natural phenomena--Prelli has identified for scientific discourse four typical categories of topoi and the line of argument relevant to each that scientists may include to enhance the persuasiveness of their discourse: problem-solution topoi, lines of argument that address whether research should or should not be accepted because it will or will not further the problem solving concerns of the scientific community; evaluative topoi, in which the values of accuracy, simplicity, scope, and consistency are

argued; exemplary topoi, which deal with analogies, metaphors, and examples, and topoi of ethos, which address possible conflicts between the rhetor's interests and the normative goals of science (125-126).

Figure two shows Prelli's four categories of topoi and lists beneath each the relevant lines of argument that he has identified.

<u>PROBLEM-SOLUTION</u>	<u>EVALUATIVE</u>	<u>EXEMPLARY</u>	<u>ETHOS</u>
Experimental competence	Accuracy	Example	Communality
Observational competence	Internal consistency	Illustration	Disinterestedness
Experimental replication	External consistency	Analogy	Individuality
Experimental originality	Scope	Metaphor	Particularity
Predictive power	Simplicity		Skepticism
Taxonomic power	Elegance		Universality
Quantitative precision	Fruitfulness		
Empirical adequacy			
Significant anomaly			
Anomaly-solution			

Figure 2. Prelli's Categories of Rhetorical Topoi

Among the problem-solution topoi, the most relevant to the analysis of cold fusion include the topos of

experimental competence, which addresses the competence of the experimenters as well as whether the observation resulted from a scientifically conducted experiment; experimental replication, which concerns whether the results can be duplicated; and external consistency, which asks if the observation or theory corresponds with accepted knowledge. The relevant evaluative topos is external consistency, the argument that findings should fit with accepted knowledge. Also important to the cold fusion controversy are the topoi that deal with the scientist's ethos. Communality maintains the importance of intellectual participation with one's scientific community and also forbids association with the lay community; disinterestedness requires that the scientist emotionally distance him or herself from the results, findings, or observations; and universality concerns whether the scientist conducted him or herself according to widely accepted scientific standards.

Prelli's Analysis of Watson and Crick

Prelli applies his special theory of rhetorical invention/analysis to several episodes of communication, including Watson and Crick's article, "A Structure for Deoxyribose Nucleic Acid" in which they communicate their discovery of the DNA double helix. Since both Prelli and Halloran analyze this article, the opportunity exists to

examine the application of their methods and evaluate their usefulness as analytical tools concerning scientific rhetoric.

Prelli contends that the success of the Watson-Crick article resulted from "the wisdom with which the authors chose their points to address and the persuasiveness and logical relevance of the topoi from which they drew argument" (249). They did what scientists must do when attempting to persuade their peers: chose a rhetorical purpose aimed at addressing a particular issue, in this case the interpretive issue, and engaged, in order, the relevant subordinate stases-- conjectural, definitional, and qualitative--that otherwise could have prevented the audience's acceptance of the paper's claims.

Watson and Crick framed the issue concerning the structure of the DNA molecule as an interpretive stasis, one involving "ambiguities in the meanings of theoretical constructs used to account for data" (Prelli 151). The community of molecular biologists had at their disposal much empirical data and accepted theory concerning molecular behavior, but had "difficulty deciding what theoretical constructs or models accommodate" the data and theory (Prelli 151).

Having framed the rhetorical situation in the interpretative stasis, Watson and Crick then proceed, Prelli discovers, to address the subordinate stases of

conjectural, definitional, and qualitative, in that order. Watson and Crick first demonstrate that no scientifically meaningful construct existed to interpret the available evidence, thereby addressing the conjectural stasis of the interpretive framework. They next offer their own model, addressing the interpretive/definitional issue by "answering the central question: What is the model for the molecular structure of DNA?" (Prelli 240). Finally, Watson and Crick address the interpretative/qualitative issue by answering the question, Which interpretive applications of the double-helix model are more meaningful? In addressing this final issue, they demonstrated their model's reasonableness by evoking the topoi of empirical adequacy and explanatory power (Prelli 243).

Prelli concludes his analysis by making the following observations about scientific rhetoric: 1) scientists, like other rhetors, choose from "among alternative ways" of expressing themselves; 2) their choices are influenced by ideals of reasonableness on the part of their specific scientific audience; 3) "a finite set of themes, values, and criteria constrains what will be necessary and appropriate to say"; 4) such constraints "are neither irrational nor formally logical" but nonetheless provide standards by which scientific discourse is judged; and 5) these constraints are modifications of "principles of

general rhetorical theory concerning ends, points at issue, and topoi" (256-257).

Halloran's Method of Close Reading

S. Michael Halloran also examines the rhetoric of the Watson-Crick paper in "The Birth of Molecular Biology: An Essay in the Rhetorical Criticism of Scientific Discourse." Though his method of analysis is less formalized than Prelli's, he identifies the topos of elegance and addresses the ethos of Watson and Crick, as might one using Prelli's method. However, his method of what I term "close reading" focuses mainly on word choice and tone and the way these features create the ethos which Halloran claims is central to the rhetorical success of the Watson/Crick article.

Halloran contends that Watson and Crick establish for themselves a particular ethos; they "offered a model of the scientist, of how he ought to hold ideas and present them to his peers" (78), that is, "confident, personal, [and] rhetorically adept" (77). The success of their paper, claims Halloran, is a result of this ethos that they establish for themselves. Halloran relies upon knowledge of the rhetorical situation and begins his criticism with a brief summary of the competition between Pauling, Wilkins, Franklin, and Watson and Crick surrounding DNA research, indicating that no one anticipated the implications of knowing the DNA structure. Halloran argues that the

Watson-Crick ethos is a result of this competitiveness and the significance of the structural model in "the possibility of mastering and ultimately manipulating the process" of genetic replication (71).

Early in his analysis, Halloran identifies Watson and Crick's use of topoi, specifically that of scientific elegance (as Watson observed, "a structure this pretty [the DNA double-helix] just had to exist") and explanatory power. (These observations are similar to those which Prelli might make and endorse.) Both topoi are represented implicitly, however, with no explicit, lengthy discussion or explanation.

According to Halloran, these topoi constitute the first two of three substantial arguments which Watson and Crick advance. The third argument is a negative one: "the proposed model is not inconsistent with any available experimental data" (Halloran 74). Though this argument is advanced explicitly, unlike the first two, Halloran emphasizes "how carefully qualified the statement is" (74). He suggests that the paper is thus argumentatively understated, "and the rhetorical effect is to communicate a sense of supreme confidence" (74). The paper's initial claims to "considerable biological interest" are advanced by "arguments [that is, topoi]...assumed to be so persuasive that they need no bolstering or emphasis" (74). Watson and Crick are able to project, then, an ethos of

supreme confidence. Thus, Halloran identifies Watson and Crick's use of the topoi of elegance and explanatory power, as one might by using Prelli's method, but Halloran's "close reading" moves him beyond identifying the relevant line of argument to revealing how the argument was implemented effectively.

Halloran next looks closely at the "genteel tone" of the Watson-Crick paper, citing from the introduction, "We wish to suggest a structure for the salt of...DNA....This structure has novel features which are of considerable biological interest" (Halloran 74). He identifies the "delicate fashion" in which Watson and Crick dismiss a rival model suggested by Pauling and Corey: "In our opinion, this structure is unsatisfactory for two reasons: (1) We believe that the material which gives the x-ray diagrams is the salt, not the free acid. Without the acidic hydrogen atoms it is not clear what forces would hold the structure together....(2) Some of the van der Waals distances appear to be too small" (Halloran 74). Halloran uses knowledge of the rhetorical situation in explaining the tone of the Pauling-Corey rejection, recalling Watson's account of how he and Crick "were astonished and jubilant to find the great Pauling guilty of what they regarded as a gross error" (Halloran 74). If this account is accurate, Halloran asserts, then the paper

"reflects a rhetorical persona" written "with a bit of intentional, tongue-in-cheek irony" (74).

Halloran attends to the one-sentence conclusion of the Watson-Crick paper, "It has not escaped our notice that the specific pairing we have postulated immediately suggests a possible copying mechanism for the genetic material." He remarks that, "One can almost feel the elbow in one's ribs," and that Watson and Crick's "genteel style becomes a transparent burlesque" (74). Again, Halloran asserts that this tone creates the persona, or ethos, of Watson and Crick that is central to the paper's success.

A Working Model of Rhetorical Analysis

Clearly, the strength of Prelli's theory of rhetorical invention and analysis lies in identifying for a given situation the relevant stases and topoi that a scientist must address to render his or her discourse scientifically reasonable in the eyes of the audience. The strength of Halloran's model of close textual reading lies in revealing the subtleties of language and argument, identifying such textual features as tone, understatement, and word choice, which Prelli's method is unable to accommodate. Combining the strengths of both Prelli's special theory and Halloran's model, one can conceive of a powerful tool for rhetorical analysis. The two-fold task of the rhetorical critic in Halloran's definition, "to discover what in the

particular case were the available means of persuasion, and judge whether the rhetor managed them well or badly" (70) would seem well-served by this combination.

Of further importance is the fact that Prelli did not in the course of his analysis reveal Watson and Crick's evocation of the topoi of elegance and explanatory power, their use being critical to Halloran's analysis. A possible interpretation of Halloran's identifying those lines of argument while Prelli did not is that Halloran's reliance on language and the understatement of argument led him to notice the existence, and importance, of those topoi. Prelli's analysis focused more on the structure of the article, the important concern for him being the identification of superior and subordinate stases. It may be that combining the methods would yield more than the sum of their parts, the two methods complementarily revealing features, then, which the other may not detect.

Considering the above, the procedure used in the next chapter applies Prelli's method, identify all possible, relevant issues and lines of argument that the writers could have drawn upon; next, identifies the issues and arguments employed; and then, following Halloran's model, determines the manner in which these are addressed and the degree of success in terms of rendering the discussion scientifically reasonable to the audience.

Chapter Four: The Rhetoric of Cold Fusion

Prelli's model of rhetorical invention, with its theories of stases(issues) and topoi (lines of argument), combined with Halloran's method of close reading offer a powerful method for rhetorical analysis. Having described this method, I will now apply it to analyze particular pieces of discourse in the cold fusion controversy. My objective will be to discover how the cold fusion controversy may be considered rhetorical in nature and how understanding the rhetoric involved can increase our understanding of the controversy in particular, as well as how science operates in general.

Selection of Texts

Although both Fleischmann and Pons and Jones initially agreed to submit papers simultaneously to Nature on March 24, 1989, Fleischmann and Pons sent a brief paper to the Journal of Electroanalytical Chemistry on March 11. Following the University of Utah press conference on March 23, Jones immediately sent his own paper to Nature. Fleischmann and Pons submitted another brief paper to Nature the next day, March 24, but subsequently withdrew that paper. Fleischmann and Pons's March 11 paper then appeared in the April issue of the Journal of Electroanalytical Chemistry; Jones's paper appeared that

same month in the April 27, 1989, issue of Nature. A flurry of discursive activity followed these events, and my rhetorical analysis will focus on representative samples.

I will examine Fleischmann and Pons's first published article about their cold fusion discovery; Steven Jones's initial article about his own cold fusion discovery; several samples of scientific correspondence both in support of and critical of the Fleischmann and Pons claim; an editorial by Nature editor John Maddox; and finally one of the final published communications by Fleischmann and Pons about their cold fusion claims. These items come primarily from one source, the scientific journal Nature, for several reasons.

First, Nature was the choice of both Fleischmann and Pons and Steven Jones for their initial publications (although Fleischmann and Pons subsequently submitted their article elsewhere and had another version rejected by Nature). Secondly, because of its content of timely scientific discoveries and discussions and its weekly publication schedule, Nature became a forum for the cold fusion controversy; scientists could publish and read therein the latest breaking news about cold fusion. Finally, Nature evolved into an intriguing and varied forum, in that alongside original research articles, one could find letters and editorials, all of which could have

been read by scientists actually engaged in cold fusion research.

I depart from this forum on two instances, however, to include significant pieces of discourse. I include Fleischmann and Pons's article that appeared in the Journal of Electroanalytical Chemistry because it was the scientists' first published account of their cold fusion experiments, and it precipitated much of the cold fusion discourse. I also include one of their final articles, published in summer 1990 in the Journal of Fusion Technology, to examine the responses of Fleischmann and Pons to representative criticisms and also because the article addresses criticisms raised at one of the several conferences at which cold fusion was discussed to examine in what ways this criticism is addressed through written discourse.

While these texts are but a small sampling of the numerous papers, letters, editorials, and news items concerning cold fusion, those texts I have selected fairly represent the kinds of issues and arguments raised throughout the controversy and convey a valid sense of the debate. Significant media of cold fusion debate not addressed closely here (see above, however) are the numerous conferences and meetings held by chemists and physicists alike, many of which were identified in chapter one's timeline. Many conclusions about cold fusion claims

were apparently reached at these gatherings, but the unavailability of accurate transcriptions precluded my examining these media in a rhetorical analysis.

The Original Research

Fleischmann and Pons's Article

Fleischmann and Pons's first cold fusion publication, "Electrochemically induced nuclear fusion of deuterium," appeared in the April 1989 issue of the Journal of Electroanalytical Chemistry. The article was not published, however, as a typical research paper; their eight-page work appeared as a "Preliminary note," an atypical entry into this journal, which usually publishes comprehensive research papers. As a preliminary note, the article failed to describe the experiment in sufficient detail to allow replication to confirm its results and contained no mention of the control experiments that are typical of this type of announcement. Journal editor Ronald Fawcett nevertheless named the paper "a very significant piece of science...definitely worth publishing as a preliminary communication" (Dagani 14).

As shown in chapter one's timeline, Pons and Fleischmann had submitted a similar paper to Nature that was rejected. The paper submitted to Nature was an even shorter version than that published by the Journal of Electroanalytical Chemistry (Dagani 14; Maddox, "Cold

Fusion" 604). Pons and Fleischmann declined requests by Nature to expand the data in their paper, taking, as Nature editor John Maddox described it, "the view that they could not at the same time satisfy the referees and get on with other urgent work" ("Cold Fusion" 604). The publication of the Fleischmann and Pons article as a note does not suggest that Fleischmann and Pons were uncertain of their claim; the term "Preliminary" does not in this case indicate "tentative" or "probing," but indicates an initial, first publication. Such a circumstance suggests that Fleischmann and Pons may have been seeking to be first with news of cold fusion, claiming the phenomenon as their own. Nature editor John Maddox raises this point in his criticisms of cold fusion, examined in detail later. Whatever their motivation, however, it is the certainty with which Fleischmann and Pons assert their claim that diminished the success of their article. The authors chose an unsuccessful rhetorical framework in which to announce their discovery and, in so doing, ignored many possible lines of argument--rhetorical topoi--against the reasonableness of their claims.

In terms of issues, Fleischmann and Pons situate their research firmly in the evidential framework, which concerns questions about the existence of phenomenon. All four subordinate stases, or issues--conjectural, definitional, qualitative, and translative--may be discovered in their

article. Fleischmann and Pons establish the overall rhetorical impetus of their article as evidential/conjectural, "Is there evidence for claim X?" at the conclusion of their brief "Introduction" with the question: "In view of the very high compression and mobility" of deuterium in the palladium molecular lattice-work, "would nuclear fusion...be feasible under these conditions?" which asks if scientific evidence exists for cold fusion. This research question, however, is similar to one that might easily begin a discussion that situates itself in the interpretative/conjectural stases and would in effect ask if any existing scientific construct, specifically nuclear fusion, could explain the experimental data that followed in the article. This interpretative approach, I argue later, might have been more rhetorically successful, but Fleischmann and Pons focus instead on the evidential.

The evidential framework is an unsuccessful one for Fleischmann and Pons because even though they fail to find conclusive scientific evidence, they remain convinced that they have indeed discovered cold nuclear fusion. By not finding evidence of the fusion products that one would expect to find based on accepted, current knowledge, however, Fleischmann and Pons are then forced to posit "an hitherto unknown nuclear process or processes," and fail to address successfully in their experimental framework the

definitional stasis, "What does the evidence mean?" and the qualitative, "Which empirical judgments are warranted by the available evidence?"

The eight-page article is divided into the four sections typical of scientific papers: Introduction, Experimental, Results, and Discussion. As cited in chapter two, Medawar argues that this arrangement reinforces the notion of the scientists who posit a hypothesis--"would nuclear fusion...be feasible under these conditions?"--and then gather objective evidence until a conclusion is reached. This arrangement conforms with the evidential/conjectural framework, again which asks "Is there scientific evidence for claim X?" In such a framework, the scientists would then proceed, after having asked the research question, to set up experiments and gather evidence, which Fleischmann and Pons do.

In the next section of the article, "Experimental," Fleischmann and Pons fail to describe fully either their apparatus or its functions in order to facilitate replication by other scientists. They write only one sentence to describe the electrochemical cells containing the palladium electrodes submerged in the deuterium bath, focusing instead on how their experimental measurements of expected fusion rates were obtained. They describe four experiments. Given below are the first sentences from each of the four sections in "Experimental" that describe those

four experiments. Where one would expect to find descriptions of experimental apparatus used to generate the phenomenon, one finds instead statements about how measurements were made, demonstrating Fleischmann and Pons's concern with the evidential:

- (1) Calorimetric measurements of heat balances at low current densities...were made using....
- (2) Calorimetric measurements of heat balances at higher current densities were carried out using....
- (3) The spectrum of γ -rays emitted from the water bath...was determined using....
- (4) The rate of generation/accumulation of tritium was measured using....

By not describing fully their experimental apparatus, Fleischmann and Pons fail to address successfully the problem-solution topos of experimental replication, in which "scientific rhetors are expected to provide sufficiently detailed explanations of methods and procedures so that the experiment can be replicated accurately" (Prelli 189).

The products of nuclear fusion are well established in the scientific community: tritium, light hydrogen, helium 3, neutrons, and (heat) energy. Note that 2 of the 4 experiments involved measured excess heat. Experiment (3) involved analysis of gamma-ray spectra (gamma-rays are produced when the neutron products of fusion are captured by hydrogen ions in the waterbath), and experiment (4) measured tritium production. Of the five products of nuclear fusion, Fleischmann and Pons describe measurements

for only three. Noticeably missing is a measurement for helium. Fleischmann and Pons promise at the end of the Experimental section that results "for the mass spectroscopy of the evolved gases [the search for He]...will be given elsewhere," and this sentence is followed by a reference. The "elsewhere" refers to as yet (at the time) unsubmitted material by Fleischmann and Pons. Were one to evoke the topos of scientific corroboration, Fleischmann and Pons could be criticized for offering evidence that is yet to be established as accepted knowledge.

By situating their discourse in the evidential/conjectural framework and describing measurements for only 3 of the 5 products of fusion, one would expect, to assure rhetorical success, to see strong experimental evidence of those three measured products: heat, tritium, and neutrons. However, Fleischmann and Pons succeed in finding convincing evidence for only one fusion product.

Several features of the "Results" section are rhetorically interesting. First in this section, Fleischmann and Pons relate the results of their calorimetric measurements. In this section are two tables depicting excess enthalpy (heat) results based on current density and electrode size (and shape). In one experiment that ran in excess of 120 hours, a huge amount of heat was

liberated. Fleischmann and Pons, in this "Results" section, make an observation that would seem better suited for the "Discussion" section; they declare that "it is inconceivable that this [heat release] could be due to anything but nuclear processes." It is clear from this remark that Fleischmann and Pons's foremost concern is establishing evidence that fusion is feasible.

In the last paragraph detailing the calorimetric results, the final, three-line sentence is bold-faced. Just prior to this sentence, they had raised the possibility that introducing tritium into the bath mixture on the reactant side of the chemical equation might yield even greater amounts of excess heat. As a warning, the bold-faced sentence reports that **"even using D₂O [deuterium] alone, a substantial portion of the cathode fused (melting point 1554° - C), part of it vapourised, and the cell and contents and a part of the fume cupboard housing the experiment were destroyed."** The bold-faced text--the only to appear in the article--serves not only as a prudent, well-intentioned warning, but also calls attention to a dramatic demonstration of the amount of excess heat reported by Fleischmann and Pons. They thus establish that at least one fusion product, heat, is undoubtedly produced.

The results of experiment (3) deal with the gamma-ray spectra obtained near the waterbath that held the

electrolytic cells. When the neutrons that are produced by fusion are captured by hydrogen ions in the deuterium bath, gamma-rays are emitted. Fleischmann and Pons report that "this spectrum confirms that 2.45 MeV neutrons are indeed generated in the electrodes." The value of 2.45 micro/electron volts is the characteristic energy signature of an electron emerging from a fusion reaction. The discussion is accompanied by a figure depicting the spectrum signal-line, showing a peak at around 2.25 MeV (this spectrum figure will be criticized by Petrasso, et al, examined below). However, the results of the spectra analysis show that "the intensities of the spectra are weak," meaning that the neutron production detected by Fleischmann and Pons is much lower than would be expected were nuclear fusion actually taking place.

The results of experiment (4) also note this low neutron flux; consistent with this low neutron production is the low accumulation in the electrolyte of the fusion product tritium. Spectra analysis indicated that tritium was indeed being produced. However, these low rates of production present a problem for Fleischmann and Pons, who confront this problem in the final paragraph of the results of experiment (4). Already established are the low neutron flux and, in agreement with this low flux, the low accumulation of tritium. "On the other hand, the data on enthalpy [heat] generation would require" neutron and

tritium production in much greater amounts. In other words, to account for the excessive amounts of heat generation, one would have to conclude that the fusion reactions are occurring at rates much greater than either the evidence for neutron or tritium production indicates.

At this point, Fleischmann and Pons have reached a rhetorical impasse. Having couched their article strictly toward the evidential issue, they are confronted with the problem of maintaining their claim of cold fusion while reasonably accounting for these low neutron and tritium rates. The products of fusion--cold or hot--are well established. Since no known scientific construct can explain their data, they are forced into one conclusion: "It is evident that [known fusion] reactions...are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

In the Discussion section of their article, Fleischmann and Pons address the evidential/definitional: "What does the evidence mean?" A close look at the language in the article shows that even though their evidence does not indicate fusion, Fleischmann and Pons leave no doubt as to what the evidence does indicate: "it is inconceivable that this [heat] is due to anything but nuclear processes," so "it is evident...that other nuclear processes must be involved," and finally that "the bulk of the energy release is due to an hitherto unknown nuclear

process or processes." In other words, Fleischmann and Pons are certain that room-temperature nuclear fusion is occurring, but in order to account for the low neutron and tritium production, they must claim further a nuclear fusion of an unconventional process. A claim of cold fusion is remarkable enough, but the Fleischmann and Pons claim is made even more extraordinary by their claiming an altogether new type of fusion reaction that yields fewer neutrons and less tritium than conventional physics allowed for. Rhetorically, then, their article fails because they have followed a framework that does not allow them to reasonably conclude that they have found evidence for what they claim to have observed.

Fleischmann and Pons introduce the evidential/qualitative issue, which asks if the evidence proves a conclusion or merely suggests areas for further study, when they suggest further study: "evidently, it is necessary to reconsider the quantum mechanics of electrons and deuterons in such host lattices [palladium]." However, Fleischmann and Pons suggest such research not because they feel their findings are inconclusive and that further research might suggest a means whereby their conclusion may find a fit with explanatory frameworks. They are actually suggesting further research so that explanatory frameworks may fit with their conclusions. As for evidential/qualitative concerns, Fleischmann and Pons have at best tentative

evidence for a new type of nuclear fusion, and one could argue strongly that the evidence is not conclusive but at best may suggest probability. Again, their language, given above, suggests they feel otherwise.

The evidential/translative issue, which asks which evidence more reliably grounds claims about what does and does not exist, is raised implicitly by Fleischmann and Pons when they base their claim on the excess heat generation. For them, "it is inconceivable that this [excess heat] could be due to anything but nuclear processes." Obviously, they consider the evidence of heat the most important indicator, with neutron flux, gamma-ray production, and tritium generation considered subordinate proofs. Recall that helium measurements were not given in the article at all. This reliance on excess heat as their primary evidence left Fleischmann and Pons open to much criticism on those grounds. Also, the evidential/translative reliance on heat evidence plays a central role in other aspects of the cold fusion controversy and will be raised later in the analysis of Steven Jones' article.

Halloran has stated that "the job of the rhetorical critic is to discover what in the particular case were the available means of persuasion, and judge whether the rhetor managed them well or badly" (70). As demonstrated, the evidential framework was rhetorically unsuccessful for

Fleischmann and Pons because inconclusive evidence prevents them from reasonably asserting the claim of cold fusion. However, as suggested earlier, another framework, the interpretative, might have promised more success, allowed Fleischmann and Pons to retain much of the information in their original article, and would have maintained their claim to the cold fusion discovery.

Fleischmann and Pons's question at the end of their "Introduction" asked, in effect, "Under these conditions, would cold nuclear fusion be feasible?" In order to reasonably answer that question, one would duplicate the conditions (compressed deuterium in a palladium molecular lattice-work) and evaluate the evidence. In the case of fusion, the products, or evidence, of fusion are well known. Fleischmann and Pons did indeed write their article along such lines. But although the evidence was not conclusive, they remained convinced that the answer to their question was "Yes, cold nuclear fusion is feasible--but of a 'hitherto unknown' kind." The question Fleischmann and Pons should have asked, however, is this: "Could cold nuclear fusion be a possible explanation of what happens under these conditions?" This question then moves the framework out of the evidential and into the interpretative: "Is there a scientifically meaningful construct for interpreting evidence?" Fleischmann and Pons could then have proceeded to present their evidence of

excess heat, low neutron flux, and low tritium generation as evidence that something unknown is happening that cannot be explained by existing scientific constructs regarding nuclear fusion. Fleischmann and Pons could then have offered that perhaps some unknown cold fusion reaction is taking place.

The difference between the two stances, though subtle, are rhetorically powerful. In their article, Fleischmann and Pons focus on their evidence as convincing proof of an unknown nuclear process, their paraphrased point claiming "We have conclusive evidence of an unknown nuclear process." More rhetorical success might have been theirs had they done something similar to what Watson and Crick did: "We would like to suggest the possibility that cold nuclear fusion may be an explanation of the excess heat phenomenon...." The original, evidential stance invites confrontation: Is the evidence conclusive? What does the evidence really mean? Are other explanations just as likely? Could chemical processes explain the phenomenon? One could also argue that the Fleischmann and Pons conclusion violates Ockham's razor, the philosophical principle that states that when two or more explanations are possible, the simplest is the most desirable. The most simple explanation of Fleischmann and Pons's results is that the heat is due to some chemical, and some known, reaction, and not to an unknown nuclear reaction.

A letter of July 1989 to Nature by Bridge et al., "Cold fusion ideas," suggests just that idea. "In the excitement" about cold fusion, they fear that "more conventional explanations from cognate fields" may be ignored. This line of argument is similar to the topos of simplicity, which holds that the most parsimonious interpretations are the most desirable. Again, the principle of Ockham's razor seems best to explain this line of argument.

On the other hand, by asking the interpretative question, "Could cold nuclear fusion be an explanation?" Fleischmann and Pons would seem both more scientifically reasonable, that is, asking if unexpected evidence could be explained by a particular construct, and would also elicit less of a confrontational reaction and more of a cooperative action. Having been given the evidence and asked the question, the scientific community could then join in with Fleischmann and Pons to determine conclusively if in fact nuclear fusion were taking place.

Fleischmann and Pons seemingly invoke this spirit of cooperation in the opening sentence to their Discussion section as they write, "We realize that the results reported here raise more questions than they provide answers, and that much further work is required on this topic." This first sentence is somewhat ironic in that Fleischmann and Pons could not have imagined just how many

questions and what sorts--concerning not only their data and conclusions but also their methods and status as credible scientists--would be raised by both the lay and scientific community. In many cases, however, an important job of the scientist is simply to raise appropriate questions or suggest fruitful avenues of research. In Lakatos' philosophy of science, such activity may be essential for the success of a research program. In this case, however, it seems unlikely that Fleischmann and Pons's intent, even though their article did result in a flurry of cold fusion experimentation, was to indicate important avenues for further study. In fact, Fleischmann and Pons were widely criticized for the secrecy surrounding their experiments, their data, and their apparatus, as will be discussed later in the analysis of John Maddox's Nature editorial. Note, too, that in the interpretative framework Fleischmann and Pons could have related their experimental findings essentially as in the original article and could have been given credit for the discovery as well, had further evidence accumulated.

The Steven Jones Article

Steven Jones of Brigham Young University is the other primary researcher in the cold fusion controversy. His article appeared the same month as the initial Fleischmann and Pons article in the April 27 issue of Nature, a little

more than a month after the University of Utah press conference. In his "Observation of cold nuclear fusion in condensed matter," Jones also situates his discourse in the evidential framework; accordingly, to be successful at convincing as to the reasonableness of his claims, he must provide scientifically reasonable empirical evidence and draw reasonable conclusions. In doing so, Jones addresses the subordinate issues of conjectural and qualitative.

Jones addresses the conjectural issue by offering neutron production as his sole source of direct evidence. No mention is made of any observations of excess heat crucial to the Fleischmann and Pons claim, and no measurements were made for helium and tritium. Helium and excess heat are discussed, however, but only in terms of "indirect evidence."

Jones describes that he arrived at his particular formula for making cold fusion from observations of "naturally occurring" Helium 3 being vented from the earth through volcanoes and fissures. If this Helium 3 were a sign of fusion, it would explain why the interior of the earth is hot, and could also explain why other planetary bodies, such as Jupiter, radiate more heat than they absorb.

What has all this to do with Jones's fusion experiment? In the next section, he explains that he has attempted to duplicate what may well take place in the

earth's interior in his electrolytic cells. The cells contain a concoction of various "catalytic materials" including "salts typical of volcanic hot springs." Into this soup are inserted electrodes of either palladium or titanium, both known absorbers of hydrogen. Through electrolytic compression, the deuterium molecules absorbed into the electrodes--in theory--eventually fuse.

Jones is able, then, to use helium as evidence, but indirectly and in a way that derives rhetorical strength through the interpretative/conjectural framework, which asks if a scientific construct exists that may explain evidence. The high concentrations of Helium 3 associated with volcanoes "suggests fusion as a possible source for the ^3He ." He further uses the interpretative stance when he suggests that "it is interesting to consider whether cold fusion in the core of Jupiter...could account for its excess heat." Jones has then in fact used both Helium 3 and heat as evidence, but as indirect, inductive evidence since Jones has modeled his experiment after those conditions and reports the observation of fusion. His couching such evidence in the interpretative framework averts any criticism that such fusion evidence has been ignored, but allows him to use it in a way that avoids confrontation as direct evidence of his own observations.

The lines of argument of experimental competency and replication are also addressed through detailed discussion

of the neutron device--a device developed by Jones and the only one of its kind--the contents and operation of the electrolytic cells, and lengthy explanations of how neutron measurements were made while correcting for the naturally occurring background count. Through such a detailed explanation, Jones sought to diminish the possibility of criticism from the methodological/definitional issue, seeking to illustrate the proper use and placement of measuring instruments and care taken while gathering measurements.

However, Jones could still not escape criticism on this issue. Immediately following Jones's article in Nature, article referee John M. Carpenter commented on the problems of measuring neutron production over background count. Though Jones and his colleagues had acted correctly to subtract background readings of naturally occurring neutron radiation--mostly in the form of cosmic-rays--Carpenter suggests that the background count can vary greatly over time. Further, the energy of such "cosmic-ray-induced neutrons is at nearly the same energy as that expected from deuteron-deuteron fusion, 2.45 MeV."

This line of argument is also used by Nature editor John Maddox in his criticisms of cold fusion in an editorial that appears in the same issue as Jones's article. Maddox laments two facts: one, that Jones had not foreseen the "need for contemporaneous controls" to

limit the effects of changing background counts and, two, that the Brigham Young neutron detector is one of a kind. Maddox undermines Jones's cold fusion work by questioning the collection of neutron data and pointing out that replication of the Jones experiment is not possible. Interestingly, Maddox points out that Carpenter "provided... [his] comment at our invitation." No other article that appeared in that issue of Nature was accompanied by the referee's comments. The Maddox editorial is analyzed in detail below.

Having presented his evidence for cold fusion, Jones concludes with the claim that "the presence of a fusion neutron signal was consistently reproduced" through several runs of the fusion experiment. He calls for more work to "disentangle the factors that influence the fusion rate." He also evokes the fruitfulness line of argument in his final sentence, emphasizing that even though the observed effect was small, it "opens the possibility, at least, of a new path to fusion energy."

The analysis of this article shows Jones's awareness of the need to structure his discourse to help render his claims reasonable and identify and disarm possible criticisms. Jones skillfully used the interpretative framework to include indirect evidence for his cold fusion claims, included detailed description of his apparatus in the attempt to diminish criticism, evokes a spirit of

cooperation to sort out factors in this new phenomenon, and points out, though cautiously, implications for future applications. In this circumstance, Jones seems the best rhetor compared to Fleischmann and Pons, and indeed his article and claims drew less criticism.

The Issue of Evidence

One other issue implicit in both the Fleischmann and Pons and the Jones paper that informs the cold fusion controversy involves concerns about which evidence more reliably grounds the claims to cold nuclear fusion. While Fleischmann and Pons based their conclusions almost entirely upon excess heat, Steven Jones grounded his fusion claims on neutron detection. Fleischmann and Pons drew much criticism about this emphasis on excess heat, especially from physicists. It is not surprising, though, that Fleischmann and Pons, being chemists, would be more concerned with the heat results than with either neutron flux or gas spectrum analysis. Heat production falls more in the realm of chemistry; physics deals more with particle detection and spectrum analysis. Recall from the timeline that Clayton Callis, ACS president, remarked that, "it appears chemists have come to the rescue" of the physicists who had so far failed in attempts at cold fusion ("Scientific" 605). At a seminar in Geneva, Carlo Rubbia, a Nobel Prize winner for work in elementary particles,

remarked that "this was the first time that a chemist had discovered a neutron" (Peat 91).

This major feature of the controversy involves arguments along the evidential/translative stasis and pits chemists against physicists: one group looked for evidence in a typically chemical product, heat, while another looked at physics products, particle production and gas spectrum analysis. During the rapid exchange of confirmations and refutations that soon followed the reports of cold fusion, "it sometimes seemed the physicists were accusing the chemists of not knowing how to detect a neutron or properly measure a gamma ray, to which the chemists would respond that the physicists did not understand how to run an electrochemical cell" (Peat 77).

Some confusion and miscommunication across disciplines should be expected, as suggested in chapter two in the discussion of Kuhn's disciplinary matrix, in which each discipline holds its own models and values for establishing knowledge, and also by Lyne and Howe, who predict miscommunication when scientists communicate with others outside their field. While no evidence of miscommunication was found, the analysis suggests that scientists may fail to identify properly the issues of concern from disciplines outside their own and thus fail to address these issues satisfactorily.

Scientific Correspondence

Cold Fusion Support

Several letters appeared in the same issue of Nature in which Jones's article appeared, two of which address the problem of low neutron flux in Fleischmann and Pons's evidence. Both, interestingly, address the interpretative stasis.

One, by Peroni Paolo, begins, "From the newspaper accounts, the very small flux of neutrons generated during the experiment of Fleischmann and Pons is being taken as proof that their conclusion is not valid" (Paolo 711). He addresses the interpretative/conjectural issue by offering a process "first recognized by Oppenheimer and Phillips in 1935" that could explain the low neutron flux; that is, Paolo suggests a scientifically meaningful construct for interpreting the low neutron flux measured by Fleischmann and Pons: "when the kinetic energy is as small as in their [Fleischmann and Pons's] experiment...the nucleus of the target atom repels the proton but not the neutron. Thus, the neutron can be captured by the nucleus while the proton...will fly off....It follows that if the experiments described really brought the deuterium nuclei close enough together to interact, one should expect no neutron emission and a reaction rate much higher...."

The other letter, by Francesco Premuda, also addresses the interpretative/conjectural stasis. His letter begins,

"Reports of the experiments by Fleischmann and Pons contain a paradox--that, if fusion reactions do occur in them, either too much energy is liberated or too few neutrons are detected. I wish to supply a possible explanation."

Premuda goes on to offer his "hypothesis" that some regions within the palladium latticework are more dense than others, inhibiting particle movement; the product particles of fusion will then "remain trapped inside....[and] the reactive particles such as neutrons and ^3H will only infrequently be released to the environment." In terms of the interpretative/conjectural issue, Premuda also offers a scientific construct by which the Fleischmann and Pons interpretation of the low neutron flux may be considered scientifically reasonable.

Cold Fusion Criticism

Fleischmann and Pons, of course, were not without their critics. Petrasso et al. submitted a letter that appeared in the May 18, 1989, issue of Nature (Petrasso, "Problems"). In the June 29, 1989, issue of Nature, Fleischmann and Pons reply to these criticisms, and this letter is followed immediately by a reply by Petrasso et al. This exchange of correspondence offers an excellent opportunity to analyze the type of discourse involved as scientists argue. In this particular series of exchanges, the discussion never moves beyond the evidential/

conjectural issue, arguing about whether evidence for fusion exists, and never into the definitional (what does the evidence mean) or the qualitative (which judgments are warranted by the evidence) or the translative (which type of evidence reliably grounds the claims).

The first letter by Petrasso et al. (Petrasso, "Problems") addresses the evidential/conjectural, primarily through the line of argument of experimental competency:

As compelling evidence that fusion had occurred, they [Fleischmann and Pons] reported the observation of the 2.22 MeV γ -ray line that originates from neutron capture by hydrogen nuclei....We argue that the claim of Fleischmann et al. to have observed the 2.22 MeV line characteristic of [fusion] reaction...is unfounded.

Petrasso et al. criticize the Fleischmann and Pons interpretation "on the basis of three quantitative considerations." First, the peak in the gamma-ray signal line at 2.22 MeV is far too narrow, for two reasons. One, based on gamma-ray spectra equations, the width of the reported peak "is a factor of two below the predicted value." This criticism raises the line of argument of external consistency, which demands that observations correspond with accepted knowledge. Two, the signal line is too narrow when one considers the resolution that Fleischmann and Pons's detector would allow. Given their instrumentation, Fleischmann and Pons could not have observed a peak of such a width at the 2.22 MeV wavelength.

Second, Petrasso et al. assert that the peak trace had the wrong profile for a fusion gamma-ray. Missing from the published signal line is "clearly defined Compton edge," a characteristic smaller peak produced when gamma-rays undergo a characteristic wavelength shift due to the Compton Effect. Again, the Fleischmann and Pons signal line interpretation fails to meet external consistency, for any such signal line with a gamma-ray peak at 2.22 MeV should show the characteristic smaller leading peak at 1.99 MeV.

Third, Petrasso et al. argue that, even though Fleischmann and Pons reported a low neutron flux, the neutron rate that they do report is "too large by a factor of 50" over what one should expect over background rate. This line of argument relies on the topos of accuracy. Thus Petrasso et al. criticize the Fleischmann and Pons evidence through the arguments of misreading instrumentation, lacking expected consistency with accepted knowledge (the Compton Edge issue), and measuring inaccurately.

Fleischmann and Pons reply to Petrasso et al. in a letter ("Measurement") that appeared in Nature some five weeks later and defend themselves and their data by quickly asserting that "the basis of the [Petrasso et al.] critique was the nature of a γ -ray spectrum displayed during a television broadcast." Also in the opening

paragraph, the scientists remark on Petrasso et al.'s "somewhat strange approach to the collection of scientific data," and contend that, in fact, the gamma-ray spectrum taken from the television and used in the Petrasso et al. letter "most certainly was not made in these laboratories." They go on to conclude that a "curious structure" in the spectrum criticized in the Petrasso et al. letter "is simply the trace of a screen cursor on the multi-channel analyzer visual display unit!" Rhetorically, Fleischmann and Pons defend themselves by attacking Petrasso et al.'s ethos, suggesting that they acted in an unscientific manner by obtaining data from an unscientific source and then using that questionable data as the basis for the critique, and by attacking Petrasso et al.'s competence by the embarrassing error of mistaking a cursor trace as a feature of the spectrum. They then produce a complete set of spectra obtained from their experiments; however, in this set of spectra, the gamma-ray peak appears not at 2.22 MeV as in their original publication, but at 2.496 MeV. Fleischmann and Pons go on to acknowledge some problems "underlying the interpretation of these spectra," but contend that "removal of the [fusion] cells leads to the removal of the signal peak" and that this evidence, in and of itself, is strong evidence of fusion.

As for the lack of the Compton edge, Fleischmann and Pons conclude that "the size and energy of the signal peak

imply that any associated Compton edge...will be lost beneath the rest of the spectrum."

In the reply, Petrasso et al. ("Reply") focus on two aspects of the Fleischmann and Pons letter. First, Petrasso et al. defend against the attack on ethos raised by Fleischmann and Pons, maintaining that "they...claim, erroneously, that their televised γ -ray spectrum was the basis of our analysis." Petrasso et al. point out that their critique was actually based on quantitative analysis of the signal lines first published by Fleischmann and Pons, controlled neutron-emission experiments that yielded gamma-ray spectra that were used as a basis of comparison for the Fleischmann and Pons spectra, and the known properties, specifically the resolution, of neutron detection instruments. Petrasso et al. successfully defend themselves against the questions of ethos by giving detailed accounts of the reasoning behind their critiques based on evidence unrelated to the televised spectra.

Petrasso et al. charge further that Fleischmann and Pons "fail to address our key criticisms" and then point to the unexplained change in signal line from the original claim of 2.22 MeV to 2.496 MeV, contending that "unfortunately, they [Fleischmann and Pons] are unable to identify the nuclear process that generates this 2.46-MeV γ -ray," or to account for its unusual profile. Petrasso

et al. conclude that the "signal line is an instrumental artefact unrelated to γ -ray interaction."

Petrasso et al. place their criticism squarely in the issue of evidential/conjectural, attacking the foundation of the Fleischmann and Pons claim. Because Fleischmann and Pons chose to argue their claim along evidential/conjectural lines, the merit of any claim they make is seriously undermined by questioning their evidence, evidence that at first was inconclusive and then, as seen above, seems to inexplicably shift.

The Nature Editorial

As suggested earlier, one of the interesting aspects of Nature as a forum for scientific discourse is its diversity. One can find therein original research articles, scientific correspondence, such as the Fleischmann and Pons/Petrasso exchange above, scientific news, and also scientific views. An example of the latter appeared in the April 27, 1989, issue as a full page editorial by editor John Maddox, entitled "What to Say about Cold Fusion." The Maddox editorial is an important piece of discourse to the cold fusion controversy because it evokes many of the lines of argument against cold fusion claims which are inappropriate in research articles of such hard sciences as electrochemistry and physics. Nonetheless, the Maddox editorial injects these arguments into hard

sciences discourse because its editorial opinion is published alongside original, refereed research.

Therefore, one must conclude that such editorials influence the scientific community's thinking and attitudes towards the research being published there as well.

For example, a tag line beneath the heading of the Maddox editorial reads "Public interest in recent excitements is to be welcomed, especially if it does not turn to anger when attempts to replicate the observation of cold fusion fail--the most probable outcome." It is obvious that the Nature editorial, just from this one tag line, casts serious doubt concerning claims of cold fusion. The implications on the community's discourse becomes even more apparent considering the possible influence on subsequent letter-writers such as Petrasso, above (whose initial critique of the Fleischmann and Pons gamma-ray data would not appear for another month; the Fleischmann and Pons reply/Petrasso reply would not appear for another two months), and even more so when one considers the impact of this editorial on the possible reaction to Steven Jones' article: the Maddox editorial appears on page 701; Jones' article appears on page 737--of that same issue.

In "What to say about cold fusion," Maddox's discussion invokes several lines of argument. He praises institutions and practices that have upheld the standards and values shared by the scientific community, and he takes

to task Fleischmann and Pons, and to a lesser degree Steven Jones. While re-affirming specifically scientific values and practices, he demonstrates how cold fusion's primary researchers have violated scientific conventions and behavior.

Maddox begins by complimenting Fleischmann and Pons on having "done at least one great service for the common cause," tongue-in-cheek praise for their having raised public interest and curiosity in the sciences and notes the "remarkably good-humoured" questions raised by the lay public about the truth of the cold fusion claims. Maddox finds it "remarkable that so many people are willing to accept that experimental observations, and the inferences drawn from them, acquire validity only by replication," and here raising the first topos and making his first affirmation about the process of science.

He credits in particular the "daily press, which has risen superbly to the challenge of cold fusion," that challenge being discussing cold fusion "in cautious language, making it plain that cold fusion was not then a proven reality." This line of argument, of course, appeals to the value of experimental replication. Some newspapers are praised for having demonstrated both "zeal and sobriety," characteristics that would also reflect well on a scientist's ethos.

One explanation for the widespread interest, Maddox observes, "seems to be the general delight that a couple of people in widely separated universities have used their own money to pull off a trick on which governments have lavished huge sums of money in the past 30 years, so far without result." In this paragraph, the tone and attitude of Maddox are evident that he uses throughout the article. Maddox seems to be appealing to some sense of rooting for the underdog, a victory for the little man over big government, resulting in "a general delight." Note also that in keeping with this facetious tone and general pessimism about the cold fusion claims, he terms those claims "pull off a trick." Governments "have lavished," with all the connotations of excess, luxury, extravagance, and squander, not invested "huge sums of money" into fusion research, which suggests that perhaps the huge sums of money have been somehow wasted. Fleischmann and Pons are referred to as "a couple of people," stripping them of their scientific status.

More explicit criticism of both Fleischmann and Pons and Jones is made through the topos of experimental competence, a recurring line of argument in the cold fusion controversy. Because of the cold fusion controversy in general, Maddox argues, "the scientific community's reputation is vulnerable in several respects...not the least of which is that neither...group... carried out the

rudimentary control experiment of running their electrolytic cells with ordinary rather than heavy water." Maddox wonders how one would ever explain this oversight to the students who have been trained "that control experiments should be as conspicuous in the design of an investigation as those believed to display the phenomenon under study," and how to "explain the neglect...to the world at large."

Maddox next attacks the ethos of Fleischmann and Pons through the topos of disinterestedness, which questions the legitimacy of scientist's motives. The line of argument follows that credible scientists set aside their own interests and are "devoted to pursuit of 'science for science's sake'" (Prelli 132). Maddox argues that Fleischmann and Pons, and Jones, have instead put at risk the reputation of the scientific community in favor of claiming cold fusion for themselves. Further, Fleischmann and Pons's "self-imposed secrecy" has hampered attempts at replication, and the lack of control experiments is a "glaring lapse from accepted practice" that is "another casualty of people's need to be first with reports of discovery and with the patents that follow." Maddox here makes explicit the suspicion abroad in the scientific community and surmised in numerous science-news journals that Fleischmann and Pons rushed into publication in order to secure patent rights.

In citing "people's need to be first with reports of discovery," Maddox implicitly reminds the reader of the Fleischmann and Pons news conference, an action that can be criticized by the topos of communality, that is, of involving members outside the scientific community in scientific matters. Had Fleischmann and Pons followed scientific convention, their discovery would have been announced in a refereed journal. Prelli suggests that "scientists denounce with special severity the scientific reasonableness of research" if the scientists appear to be in "complicity with the laity" (Prelli 132).

This "need to be first" also opens another line of analysis that enriches Maddox's editorial. Pons and Fleischmann's initial article in the Journal of Electroanalytical Chemistry was not published as a typical research paper; their eight-page work appeared as a "Preliminary note," an atypical entry into this journal. As such, the article failed, as Maddox has pointed out, to describe the experiment in sufficient detail to allow replication to confirm its results, and contained no mention of the control experiments that Maddox has correctly argued are typical of this type of announcement.

As stated in chapter one's timeline, Pons and Fleischmann had submitted a similar paper to Nature that was rejected. The paper submitted to Nature was an even

shorter version than that published by the Journal of Electroanalytical Chemistry (Dagani 14; Maddox, "Cold Fusion" 604). Pons and Fleischmann declined to expand their paper, taking, as Maddox put it, "the view that they could not at the same time satisfy the referees and get on with other urgent work" ("Cold Fusion" 604). Maddox carefully indicated at that time, however, that "the non-appearance of the [Fleischmann and Pons] article must not be taken to imply that the experiments...are inherently less believable than those of Jones" (whose article was, of course, accepted by Nature) ("Cold Fusion" 604).

In this editorial, however, Maddox makes it clear that "the Utah phenomenon is literally unsupported by the evidence, could be an artefact and, given its improbability, is most likely to be one." It is unclear what has happened in the interim that would explain so radical a change in position.

However, the theme of "improbability" is evoked twice by Maddox. Earlier in this same editorial, he admits that "the Fleischmann and Pons experiments raise bigger questions, if only because the scale of the phenomenon they report is so much greater." Here, Maddox argues that "given its improbability," the claim is most likely false because it claims too much. If Maddox is reflecting a theme at large in the general scientific community, one may term it the topos of plausibility, in which case the lay

connotations of reasonable--plausible, likely, moderate, and sensible--would translate into scientific reasonableness. If such is the case, it explains one of the reasons why Jones' claims, in which the fusion rates are much lower than Fleischmann and Pons, are the more well accepted and less criticized. Jones' claims would be considered the more scientifically reasonable because they suggest less disruption to accepted knowledge concerning nuclear fusion than Fleischmann and Pons's claims, which require proposing a new type of nuclear fusion.

Fleischmann and Pons's Response

One of the final communications of Fleischmann and Pons detailing their cold fusion experiments was published in the July 1990 issue of the Journal of Fusion Technology. Here, they "enumerate...the major criticisms that have been made and compare them to our actual procedures." They enumerate seven major criticisms that have questioned their competence as experimenters. Interestingly, Fleischmann and Pons seem more concerned about the criticisms that have arisen from press accounts than those raised in scientific forums (the latter receive only parenthetical comment): "there have been numerous comments in the press (and some in the scientific literature) about the accuracy of our methods, and, therefore, the validity of our results." Of course, Fleischmann and Pons are acutely aware of the

critiques directed towards them over the evidential/conjectural issue. Following is an analysis of their responses to their critics.

Responses II.A. & B. confront criticisms along the lines of experimental competence that question the care with which Fleischmann and Pons handled the gases that issued from the electrochemical cells and whether purity was maintained in the deuterium water bath. Fleischmann and Pons maintain that great care was taken on both counts through the design and close monitoring of their apparatus.

Response II.C., "Inadequate Mixing of the Electrolyte Leading to Marked Temperature Differences in the Cell," addresses the arguments of experimental competence made by Cal Tech chemist Nathan Lewis at the May meeting of the American Physical Society held in Baltimore. Working with a physicist and several coworkers, Lewis built a model of the Fleischmann and Pons apparatus. Since no details of the apparatus had been given, Lewis constructed the fusion device "using press photos and estimating dimensions from human arms in those photos" (Dagani 12). Lewis reported having performed several cold fusion experiments, finding no evidence of neutrons, gamma-rays, tritium, helium, or excess heat. Lewis did discover, though, the ease of committing errors in running such experiments, one of which was unreliable heat readings if the electrolyte in the cells is not stirred ("Secret Life" 88; Lindley, "More than

Skepticism" 4). Lewis said that the precision of heat measurements is "highly dependent on exactly where you place the thermometer and how well stirred the electrolyte is" (Dagani 18). This criticism is especially relevant because, in this case, it is a chemist telling Fleischmann and Pons that they have not run their electrolytic cell properly.

This criticism of experimental competence is situated in the methodological/definitional framework because the issue concerns the proper operation of an electrolytic cell in which heat measurements are made. Fleischmann and Pons reply to this criticism by explaining that stirring is accomplished by the bubbling solution; in effect, the electrolyte stirs itself through "gas sparging," and a dye added to the solution demonstrates a quick dispersal throughout the cell.

Response II.D., "Inadequate Control of Water Bath Can Produce Errors," is interesting in that "although this aspect does not appear to have been raised in the literature, we comment on it here as a possible critique of our and other work." This response shows Fleischmann and Pons as practicing scientific rhetors, here attempting to reduce the possibility of criticism along the lines of experimental competence. They focus on what they consider the important details of their cell apparatus and describe briefly the pumps and mechanism used to maintain water

levels, and then point out that "the control that we achieved may be contrasted to that in other published work, for example, $\pm 0.08^{\circ}\text{C}$." Fleischmann and Pons list their temperature fluctuations at "less than $\pm 0.003^{\circ}\text{C}$," thereby demonstrating that they maintained stricter control in their cells than many of their critics.

Fleischmann and Pons's sections II.E. and II.F. again address the methodological/definitional issue, arguing the proper application of and use of values in chemical equations used for measuring heat transfer.

Section II.G. reveal Fleischmann and Pons's reaction to Maddox's editorial, reviewed above: "We believe the allegations in Nature that we had not carried out blank [control] experiments before the publication of our preliminary note have been one of the principal factors preventing a logical development of research in this area and in polarizing attitudes." Of rhetorical importance is Fleischmann and Pons's clear perception that lines of scientific research have been hampered, not by lack of merit of either their evidence or their claims, but by allegations made in an editorial that questions their failure to maintain experimental competence and uphold the values of science. Not only has research been impeded, but evidence exists that scientists have taken sides on the issue, based, again, not on the merits of claim making or evidence but failure to include descriptions of control

experiments and apparatus that would allow others to replicate the experiment.

In answer to Maddox's criticism, Fleischmann and Pons indicate that "these allegations are difficult to understand since the preliminary publication did in fact contain one blank experiment....Our view has always been that...[a cell that does not show excess heat] is the most appropriate blank." Perhaps Fleischmann and Pons's definition of control experiment differs from Maddox's. Maddox called for an experiment conducted in light water instead of heavy, deuterized water. Fleischmann and Pons consider the control to be a cell that shows no excess heat, against which cells that do exhibit heat may be compared. This issue, the methodological/translative, arises over concerns about which experimental methods will yield the best evidence. Fleischmann and Pons are satisfied that blanks are appropriate for controls; Maddox feels otherwise. In either case, this dispute has been responsible for much of the controversy involving cold fusion research. Fleischmann and Pons conclude by generally dismissing "most of the speculations" about their experiments and conclusions as "exaggerated at least."

A study of the rhetoric involved in this controversy suggests that Fleischmann and Pons had at their disposal the rhetorical means to avert the nature of many of those speculations. A paper with the content of their last, but

published earlier--and so without the need for the defensive posturing--might have averted much damage to the credibility of both Fleischmann and Pons and cold fusion research generally. Fleischmann and Pons also demonstrate a seeming unawareness of many issues and lines of argument that they fail to address until much damage has been done to the line of research, not the least of which are experimental competence and replication. The rhetorical analysis further demonstrates that philosophical and sociological aspects enter into the scientific discourse. I examine the implications of these observations in the next chapter.

Chapter Five: Conclusions and Implications

On March 18, 1989, Martin Fleischmann and Stanley Pons touched off a scientific controversy by announcing at a University of Utah press conference the discovery of room temperature nuclear fusion, which promised an abundant source of clean, cheap energy. This dissertation attempts to discover in what ways this cold fusion controversy may be considered rhetorical in nature and in what ways understanding the rhetoric involved can increase our understanding of the controversy in particular, as well as the way science operates in general. To accomplish this task, I have summarized the circumstances surrounding the controversy, reviewed relevant literature to establish lines of thought that suggest a rhetorical element in the practice of science, synthesized a working theory of rhetorical analysis, and applied that theory to discursive artifacts arising from the controversy.

Evident in this discourse is the rhetorical element in the practice of science; considering this element, I venture here a definition of scientific rhetoric: the activity of science whereby knowledge claims and ideas are disseminated and mediated among scientists in an attempt to establish the soundness of those claims and ideas. In the occurrence of conflicting evidence for cold fusion, the scientists, through their discourse, exchanged the

findings they deemed relevant, addressed the issues they determined that needed attention, and raised lines of argument they found central to the investigation, all in the attempt to determine the soundness of Fleischmann and Pons's claims.

The exchange of letters in Nature concerning cold fusion indicate that science involves more than gathering empirical evidence and interpreting findings. Scientists frequently exchange information and debate about which scientific constructs are more fruitful for application, if existing constructs can explain unusual or unexpected data, the competence of experimentation, and so on. Fueling the cold fusion controversy was the fact that although experiments are typically replicable, cold fusion cells sometimes seemed to work and sometimes did not; some labs reported some evidence in some cells; some labs would see evidence sometimes; and others reported nothing. However, failure to detect fusion did not disprove the Fleischmann and Pons claim, for as one journalist observed, "the absence of evidence is not evidence of absence, and negative results are as open to doubt as any others" ("The Secret Life" 90). Thus, the mediating role of rhetoric is made evident as scientists attempt to agree, in this instance, at what point falsification is reached. Further, such exchanges provide evidence of the critical discourse that Popper suggested was so central in arriving at

"truth," demonstrating rhetoric's central role in disseminating and mediating knowledge claims among scientists.

This definition also accommodates the concept that while a rhetoric of science is not a philosophy, sociology, or history of science, it agrees with such. This rhetoric is an overarching element that draws upon and is inextricably bound to the philosophy and sociology of science, as well as procedures for inquiry and knowledge production, the concerns of the rhetoric of inquiry. Two elements that appeared in the cold fusion discourse demonstrate this overarching nature of scientific rhetoric.

First, a line of argument that I identified and termed "plausibility" appears in the cold fusion discourse and may reflect a value held at large by the scientific community. In this line of argument, the lay connotations of reasonable-- plausible, likely, moderate, sensible--would translate into scientific reasonableness. For instance, Maddox argues that "given the improbability" of Fleischmann and Pons's claims, they are likely false (604). Such a line of thought offers one explanation of why Jones was received more positively than Fleischmann and Pons, for his claims of fusion were more modest by a factor of ten and suggested less disruption to accepted knowledge.

Second, related to but not identical to this argument of plausibility is what I term "scientific inertia," the seeming reluctance of scientists to accept new claims that may seem far afield of claims allowed by current lines of thinking and investigation. Kuhn has pointed out, for instance, that when current theory fails to explain new evidence, the individual scientist's own ingenuity and apparatus are questioned, not the shared values and assumptions of the specific scientific community; Lakatos posited the existence of the "hard core" of scientific knowledge that is not subject to modification, even though research programs may shift lines of investigation. Further evidence of what I termed scientific inertia is found in the concluding item of a 1989 Department of Energy Energy Research Advisory Board (ERAB) report on funding for cold nuclear fusion. Though the ERAB reported that "it is not possible at this time to state categorically that all the claims for cold fusion have been convincingly either proved or disproved," the ERAB concluded that

Nuclear fusion at room temperature, of the type discussed in this report, would be contrary to all understanding gained of nuclear reactions in the last half century; it would require the invention of an entirely new nuclear process. (Goodwin 45).

It is clear that "the invention of an entirely new nuclear process" is not at all considered a positive circumstance. This scientific inertia is a worrisome philosophical and sociological phenomenon that is evidenced through

scientific discourse, worrisome because it apparently places barriers in the way of scientific progress and could conceivably stifle scientific and technological achievement.

The appearances of the "plausibility" line of argument and of scientific inertia demonstrate the inter-relationships of rhetoric with the philosophy and sociology of science. Scientific rhetors must look to the way science works and scientists work together in order to discover the available means of presenting their claims and ideas as scientifically reasonable. Kuhn's and Lakatos's philosophies predicted the plausibility argument and scientific inertia; the wise rhetor, then, should include in his or her discourse elements to overcome the reluctance to accept radical claims or to demonstrate the benefits of redirecting current research programs and lines of thinking. The close study of cold fusion discourse reveals elements of scientific philosophy and sociology. Therefore, the discourse may serve as well to illuminate the actual practices and behaviors of scientists. All the above conclusions have implications in the rhetoric of inquiry, whose concern is structuring human inquiry and building knowledge.

Though I maintain that science necessarily contains a rhetorical element, neither my definition nor my rhetorical analysis go so far as to suggest that science or the body

of scientific knowledge is wholly rhetorical, or, on the other hand, suggest the least that science merely includes some amount of rhetorical activity. As cited in chapter three, McMullin maintains that science has two faces, rhetoric being one. The other involves the logic of experimentation and data collection. In the case of cold fusion, had corroborating evidence never surfaced, no amount of rhetoric could have convinced the body of scientists that the process of cold fusion actually worked. The empirical aspects of experimentation and observation serve as a check and guardian against such a circumstance. Yet, asked the question, "Do scientific writers structure their discourse to accommodate relevant issues and lines of argument in order to convince their audience as to the scientific reasonableness of their claims?" one may quickly and decisively answer "Yes."

This line of argument raises definite epistemological questions. However, answering these questions is beyond the scope of a rhetorical analysis. For just as the rhetoric of science is not a philosophy or sociology, but agrees with the tenets of such, the rhetoric of science is not itself an epistemology of science, a point proven by its ability to agree with many epistemological stances. Hikins and Zagacki have shown that rhetoric is commensurable with realist positions through the theory of rhetorical perspective. Neither verificationists nor

subjectivists would argue with a rhetorical element in science. The proponents of what Berlin termed the New Rhetoric or Epistemic Rhetoric believe language is an integral part of shaping and producing knowledge. Even the rationalist Popper admits the importance of critical discourse in the search for truth:

It is only the idea of truth which allows us to speak sensibly of...rational criticism, and which makes rational discussion possible--that is to say, critical discussion in search of mistakes with the serious purpose of eliminating as many of these mistakes as we can, in order to get nearer to the truth. (Popper, "Conjectures" 229)

Popper's role for critical discussion agrees with the definition of scientific rhetoric that I offered in the beginning of this chapter: exchanging evidence, raising issues, and identifying relevant lines of argument to establish the soundness of knowledge claims. Obvious from both the rhetorical analysis of cold fusion discourse and in Popper's discussion is that scientists do engage in rhetoric while "doing" science. For scientists, however, that rhetoric is combined with the empirical logic of experimentation and observation. The question, then, of whether scientists produce "scientific truth" or "probable knowledge" is a red herring for the rhetorical critic. As suggested in chapter two, the question "Is cold fusion a reality?" differs from "Do scientists engage in rhetoric?" Further, depending on one's epistemological bent, truth and probable knowledge are not mutually exclusive concepts, so

in some contexts, such a question is not only unanswerable, but may be even, if you will, unaskable. The epistemological question that may interest rhetorical critics, however, is "How might one's epistemological stance influence one's rhetorical choice?" For instance, if rhetors see the scientist's role as producer of truth rather than interpreter of evidence, they may choose to address issues from the evidential framework more so than those from the interpretative. I suggest later that this notion may explain Fleischmann and Pons's rhetorical choices.

This line of thinking also suggests a caveat for the rhetorical critic against applying his or her own epistemological position to the text under analysis. For instance, while the Epistemic Rhetoric that Berlin describes embodies a popular position among humanities scholars, who frequently conduct rhetorical analyses, such a position may not be popular among chemists and physicists. Perhaps humanities scholars are more amenable to the notion of the pervasive nature of language because language is their tool. Chemists and physicists, however, have, as McMullin has suggested, another tool: the logic of experimentation and observation. The rhetorical critic must strive to learn how that methodology may influence rhetorical choice. To understand deeply the full range of rhetorical choices open to a scientific rhetor (or any

other rhetor, for that matter), one should be aware of that rhetor's own epistemological assumptions. Further, another task for the rhetorical critic becomes clear: just as familiarity with the philosophy and sociology of science enriches a rhetorical analysis, so too may a knowledge of various epistemological positions.

Considering the above, the goal of the rhetorical critic, then, is to examine this dissemination and mediation of knowledge claims and determine to what degree the scientific rhetor has established the reasonableness of his or her claims. Following are some specific conclusions I have reached considering the results of the rhetorical analysis of the cold fusion discourse.

For the most part, cold fusion discourse addressed the evidential issues, focusing on whether the cold fusion phenomenon did or did not exist. The scientists involved disseminated what they considered relevant evidence in order to address this issue of existence. Several lines of argument were invoked within this framework, not surprisingly those involved with the task of problem-solution and also those which address ethos, for revealing improprieties on the part of the experimenters is to raise questions concerning the scientific reliability of their evidence. Fleischmann and Pons chose to address their discourse to the evidential framework. This framework's

foremost concern is considering whether evidence exists that supports claims; Fleischmann and Pons's evidence was insufficient to persuade as to the scientific reasonableness of the claim of cold fusion. As one reporter observed, "it is more sensible to reject sporadic and dubious readings than to reject well-established theories" ("The Secret Life" 90).

In several instances, the evidential issues raised by Fleischmann and Pons's critics were translated into methodological issues, the quest for accurate and reliable data becoming the task of proper application of experimental apparatus. For instance, Cal Lewis's criticisms concerning stirring of the waterbath in fusion cells to obtain accurate temperature measurements addressed the methodological/ definitional issue through the line of argument of experimental competence. Jones attempted to reduce the possibility of criticism on this point through his lengthy description and graphic depiction of the design and implementation of his neutron detector.

Fleischmann and Pons had at their disposal other rhetorical options that might have made their claims more convincing to their audience. Had Fleischmann and Pons addressed the interpretative issue, instead of the evidential, they could have conjectured "that cold nuclear fusion may be an explanation of the excess heat phenomenon." Such a posturing would have allowed

Fleischmann and Pons to present the same evidence while maintaining their claim to the cold fusion discovery. Given the tentative nature of the evidence, this stance also might have seemed more scientifically reasonable by conjecturing that a particular scientific construct may explain the data and therefore evoked a less confrontational response from their audience. Having been presented with the evidence and asked a question such as, "Could cold nuclear fusion be an explanation?" the scientific audience could have joined with Fleischmann and Pons in determining an explanation. Fleischmann and Pons may also have been able to invoke the spirit of puzzle-solving that Kuhn suggests fills much of the scientist's experimental time.

Why Fleischmann and Pons chose to present their work in the evidential framework as opposed to the more promising interpretative is a question that cannot be answered certainly. However, I suggest here two possible explanations with some interesting implications for rhetorical choice. First, Fleischmann and Pons may have been influenced, as discussed above, by their perceptions of the scientist's role, scientist as producer of truth or scientist as arguer for interpretations of evidence. Their evidential stance is the more decisive and emphatic stance, implying the notion of scientist as producer of truth; the interpretative is the more tentative, offering possible

explanations for the evidence and depicting the scientist as interpreter of evidence. An interesting question to investigate is how epistemological stance may have influenced Fleischmann and Pons's choice.

Secondly, Fleischmann and Pons may not have addressed the interpretative issue simply because that is not how electrochemists typically communicate with other electrochemists. Electrochemistry is a more applied, less theoretical field than, for instance, nuclear physics. Perhaps Fleischmann and Pons never considered the possibility of situating their discourse in the interpretative issue because electrochemists are unaccustomed to theoretical interpretations of their evidence. Nuclear physics, on the other hand, is a more theoretical, less applied field, and physicists might be more accustomed to writing theoretical, interpretative papers. Indeed, as shown in the rhetorical analysis, Jones does address the interpretative issue when introducing evidence for his cold fusion claim. Interestingly enough, Fleischmann and Pons might have made an appropriate rhetorical choice considering both their field of specialty and that field's audience. This thought suggests that while Fleischmann and Pons may have used a stance suitable for the audience of electrochemists, they experienced some difficulty in identifying and addressing relevant issues for nuclear physicists.

The rhetorical analysis suggests also that scientists communicating beyond their specialty may fail to identify and therefore address differing concepts of what constitutes valid evidence. This finding validates Kuhn's concept of the disciplinary matrix. Fleischmann and Pons, as electrochemists, fashioned an experiment and employed apparatus intended to detect heat. Two of their four experiments were set up for calorimetric measurements, the evidence upon which they based their cold fusion claims. Jones, a physicist, constructed his own sensitive neutron detector (the only one of its kind) and proceeded to look for neutron production as fusion evidence. Because physicists considered neutron production as reliable evidence of cold fusion, Fleischmann and Pons's claims were less convincing to that audience, especially considering that the neutron rate they detected was less than normally expected for such a phenomenon. And although the physicists insisted that the low neutron rates disproved cold fusion, the chemists insisted that the excess heat production could be due only to a nuclear--not chemical--reaction.

Fortunately, no permanent rift between chemists and physicists appeared. In a July 1989 editorial entitled, "End of cold fusion in sight," Maddox happily reported that "the brief spell in April when it seemed as if cold fusion

would permanently divide chemists and physicists has left no trace" ("End" 15).

Additionally apparent is that the scientists' basic assumptions, inherent before the experiments were conducted, determined the configurations of the experiments and what evidence the scientists would look for. Considering such, one may imagine the possibility that researchers exhibiting such behavior could altogether overlook other significant phenomenon.

Further hampering Fleischmann and Pons's rhetorical success was their violation of several scientific values and standards, damaging their ethos. Not the least of these ethical lapses were bypassing the peer-review process in favor of the infamous press conference and their secrecy concerning their process and apparatus. This concern with ethos was not evident in the research articles themselves, such lines of argument seeming inappropriate for such a forum. However, these concerns are apparent in the Maddox editorial, indicating that the scientific community was aware of these violations and their possible effect on the scientific ethos in general. That the community of research scientists involved in fusion research was influenced by such editorials is easily argued, the best support being the appearance of the Maddox editorial decrying the claims of cold fusion in the same issue of Nature in which Jones's initial publication appeared. Such

communications played an important role in the discourse of this "hard science" controversy, perhaps not to the degree of research articles, but as one avenue whereby such concerns are voiced and disseminated.

The unusual circumstances involved in the cold fusion controversy offered a unique opportunity to examine rhetoric's role in scientific discourse. Whether cold fusion exists as a phenomenon is still a mystery. However, I maintain that Fleischmann and Pons had at their disposal alternative rhetorical choices that could have increased the effectiveness of their discourse. Had they followed scientific standards and approached their rhetorical situation differently, even though the claims of cold fusion were not confirmed in the laboratory, the line of research might have seemed more credible and the scientific community more energized by its possibilities.

In answering Overington's call to students of rhetoric to turn their skills to the analysis of scientific discourse, I have attempted to reveal essential aspects of the cold fusion controversy--and scientific practice--otherwise hidden to the lay observer and quite possibly to the involved scientists themselves. Informed by a background in the sociology and philosophy of science and a working theory of rhetorical analysis, I hope to have demonstrated that even someone situated far outside the

field of the hard sciences such as electrochemistry and atomic, nuclear, and particle physics can access highly specialized scientific discourse and make it intelligible.

Many possibilities and much work remain for rhetorical critics. Perhaps the most exciting possibilities lie in asking the question, "What are the specific issues and lines of argument specific to various disciplines?" Once these are identified, a matrix such as Prelli's could be constructed and used in rhetorical analysis for each. For instance, what are the issues and arguments used by psychologists? Would these differ from those used by composition theorists? A formalized method for both invention and analysis would open the possibilities for the rhetorical study of many fields.

When studying specific fields, many questions remain to be answered. For instance, how do constructs such as disciplinary matrices affect communication within disciplines? Across disciplines? Do the epistemological assumptions of that discipline's participants influence rhetorical choice? Also, rhetorical critics should begin to identify what discursive media exist for those participants other than the written medium. For instance, in the cold fusion controversy, meetings and conferences played an important role in scientists' conclusions about cold fusion. To what extent is that role rhetorical? How

do the conclusions reached at such meetings influence written discourse?

Finally, knowledge of the philosophy and sociology of science greatly enhanced the rhetorical analysis of the cold fusion controversy. This circumstance would imply that the rhetorical critic should bring to bear in an analysis knowledge beyond that of only rhetoric. In studying various disciplines, then, what types and what scope of additional knowledge must the rhetorical critic seek out to enrich an analysis?

These questions suggest that the task of rhetorical analysis is a complicated one. However, the importance of making scientific and other specialized discourses accessible to those outside the specialties is a task of increasing importance. As Overington suggests, "To understand public discourse in the closing decades of this century, we must have some understanding of scientific discourse" (81). Scientists everyday make decisions that impact more and more on public life and policy. The rhetorical critic is in a unique position to sort through the complexities of strategy and choice, identify issues and arguments, and make the process and products of scientific discourse more accessible.

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Vita

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